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ART. I.—*The Replier replied to, and the Reviewer reviewed*—A letter to Lunsford P. Yandell, M.D., containing Strictures and Thoughts on the errors and false doctrines of Chemico-Physiology. By CHARLES CALDWELL, M.D.

(CONCLUDED FROM PAGE 1.)

Having passed thus hastily over what may be regarded as comparatively minor points, I shall now submit to you a more detailed exposition of the experiments of Fordyce, Banks, and Blagden.

The object of those three philosophers was to ascertain, by experiment, the effect produced on the human body, by a temporary exposure to a very elevated tempera-

ture. And that the result of the trial might be the more accurate and satisfactory—and therefore the more valuable, they determined to experiment on their own persons.

For the accomplishment of their purpose, they prepared a suit of three rooms, each from 15 to 20 feet square, and of a corresponding height.

The atmospheres of those apartments they heated to different degrees, ranging, I think, at different times, from 100° to 264° of Fahrenheit. To these varying temperatures did they expose their naked bodies (during periods sufficient to make the trials fair) sometimes individually, and at other times in company with each other—determined, that by diversifying their experiments, under the best concerted plan of action and observation, no ground should be left for cavil or doubt, as to the truth and accuracy of the processes and their results. On no occasion, however, did their persons, even when acted upon by the hottest atmosphere, manifest a temperature higher than from 100° to 100½°, and 101° of Fahrenheit.

At that temperature did they steadily remain, even when the heat of the atmosphere around them, and acting on them, varied from 224° to 264°; and when eggs immersed in cold water and exposed to it were *boiled to hardness* in 20 minutes; and beef-steaks sufficiently *broiled* in it in 13 minutes. No painful or unpleasant sensations moreover were produced on the skin, lips, or eyes, by the portion of the highly heated air, which came into immediate contact with them; nor on the mouth, tongue, fauces, nares, larynx, trachea, or lungs, by the portion which was breathed. Nor was this all.

Not only did the burning atmosphere of the rooms fail to elevate the temperature of the bodies of the experimenters; their bodies rapidly and materially lowered its temperature.

In proof of this; into one of the rooms, at the temperature of 210°, Dr. Blagden entering alone, the thermometer in it soon fell to 198°. The temperature was again raised to 211°; when, on the entrance of Sir Joseph Banks alone, it fell again, in a few minutes, to 198°. And, on the entrance

of the three gentlemen at once, the descent of the quicksilver was much more rapid, and, consequently, much greater, in a given period. It is worthy of remark, that the more vigorous the *constitution* of the individual (by which is meant his *vital force and action*) the more rapidly did he reduce the temperature of the atmosphere, and the more slowly and to the less extent was the temperature of his own body elevated. This fact clearly bespeaks the issue to have been the product, not of *chemical*, but of *vital* influence. The vigor of that influence counteracted and held in check the tendency of the heated air of the apartment to raise the temperature of the bodies of the experimenters. But, according to your and Liebig's hypothesis, the reverse should have been the case. The greater the vigor of the constitution and its vital action, the greater is the amount of the oxygen, hydrogen, and carbon consumed, in a given time, and consequently the more intense is the burning. And this burning from within must necessarily co-operate with the heated air without, in the elevation of the personal temperature. When applied to the present case, therefore, the hypothesis you would sustain destroys itself. A strong constitution resisted the heated atmosphere, *because* it was strong; while a feeble one yielded to it, *because* it was *feeble*. This fact, therefore, is fatal to the notion of Liebig—as far at least as a single fact may avail.

Meantime, though the temperature of the experimenters was so slightly increased, their pulses rose in frequency, from their natural standard (say 75 to 80) to 150 beats in a minute. And, of course, the frequency of their respiration was also augmented, as well as the quantity of air which they breathed in a given time.

Such are some of the leading and most significant facts and their accompaniments of the case. Nor is their signification either obscure or equivocal. Far otherwise. It announces in language too clear and definite to be misunderstood, and too emphatic and forcible to be resisted, that, by a process or mode of action, utterly inexplicable on chemical principles,

the living human body can and does abstract sensible caloric from the air around it, materially reducing the atmospheric temperature, while, by throwing into a latent condition the matter of heat, thus abstracted, it prevents the increase of its own temperature.

That the human body does thus withdraw caloric from the atmosphere which encircles it, and cool that, without becoming itself correspondingly heated, is a position which you cannot deny; because the facts of the case in question directly and conclusively testify to its truth. But you contend that, in so doing, it acts on principles, and by a process essentially chemical; and that process you attempt to expound. By a few strictures however on your attempt, and on the principles and means on which you depend for its accomplishment, its entire insufficiency may be satisfactorily evinced, and its failure made plain.

In your effort to explain and remove the difficulties that press, like an incubus, on Liebig and his retainers, you say, that

"The effect" (manifested in the preceding experiments) "which strikes one as so wonderful, depends, first, upon the air's being a bad conductor of caloric, and secondly, upon the evaporation of the perspirable matter from the body."

And there, your resources of explanation being exhausted, no further effort to that effect is made by you. If I can show therefore, that in this death-struggle to maintain yourself and your hypothesis, you not only fail to accomplish your purpose, but involve yourself in further entanglements and inconsistencies, and thus render your cause more hopeless, and your prospects more gloomy—should I succeed in doing this, though I shall not presume to tender to you any advice in the premises, nothing forbids me respectfully to submit to you whether it may not be discreet in you to retire from the contest, and allow Professor Liebig hereafter to be his own champion, and to fight his own battles? Under existing circumstances, you have nothing of scientific standing or reputation

to gain; though, as relates to both, you may lose not a little. Should you, in the conflict, prove victorious, the victory will redound to the honor of Liebig; because you make battle with his principles, in his name, and under his banner. But, in case of defeat, the disaster and disrepute will fall exclusively on your own head; because, by your fellow-members of the school of Giessen, the catastrophy will be ascribed, not to any failing or fault which attaches to either Liebig, his doctrine, or themselves; but to some defect in your qualifications or conduct—in your prowess—your science—your skill in tactics—your judgment in the selection of weapons—or your dexterity in the use of them. These hints are not actually *offered* to you—especially they are not *pressed* on you—they are merely deposited where you may find them. Employ them as you may think proper, or let them remain unemployed.

In pronouncing atmospherical air 'a bad conductor of caloric,' your object I presume is, to endeavor to make it appear, that it did not readily impart that substance to the persons of Fordyce, Banks, and Blagden, and therefore failed almost entirely to heat them.

If this be the sentiment you mean to convey, permit me to observe, that it is altogether groundless. The air did impart its caloric to the persons of the experimenters very readily and copiously; because, while they were within the atmosphere of the rooms, prepared for their inquiry, its temperature fell both rapidly and extensively. When but a single individual was in contact with it, the whole volume of the air contained in the apartment lost, in a few minutes, in one case, 12° of heat, and in another 13°.

Wherefore did this fall of temperature take place? The answer is plain and not to be controverted. The air parted from its caloric, and communicated it to the body of the gentleman who was in the midst of it. Yet, by such communication, the temperature of his body was not raised. Of this again the cause is obvious. By some form of vital influence, not yet discovered, his body reduced to a latent state the

caloric it received from the surrounding air. To such a state the caloric unquestionably *was* reduced, because its influence was no longer experienced by either the atmosphere or the bodies of the experimenters.

The truth of this even you yourself substantially acknowledge; though you do not appear to be conscious of the fact. The following are your words:

"The stratum of air in contact with them" (the experimenters) "*parted with a portion of its caloric*, grew heavier in consequence, and subsided along the floor, making way for a fresh volume of hot air, in its turn to be cooled down, and settle towards the bottom."

Though unwilling to find fault with style and manner in the composition of a scientific paper, I am notwithstanding compelled to pronounce this a very labored and obscure sentence. Owing to either its entanglement and want of lucidity, or to my want of ready perceptiveness, to derive from it any clear and definite idea of the view or conception, under which you wrote it, is beyond my ability. Nor do I hesitate further to pronounce obscurity of expression a natural result of obscurity of thought. It is moreover the result of that *alone*, when its author is deliberate and careful in writing, and has a ready and felicitous command of speech. Now such command is known to be yours, in the literal and full signification of the terms I have employed in expressing it. In plainer and more definite language, and without the slightest disposition to compliment or flatter you, your store of words is unusually abundant; and you draw on it with a corresponding facility and promptness.

I am therefore convinced that, when you penned the paragraph now before me, your mind was puzzled and embarrassed from a barrenness or confusion of ideas—you labored in vain in quest of suitable and satisfactory matter—and your thoughts were misty, vague, and indefinite. Had not this been the case, your phraseology would have been, as it very rarely fails to be, clearly intelligible to me. But haplessly it

is not so. Allow me therefore to hope, that you will neither be surprised, nor deem me in a high degree faulty, should I fail in my interpretation of it.

Through the twilight of your expression, however, I am so fortunate as to discry, with sufficient clearness, one act of your mind, which is important to my purpose—your admission of the fact, that the air, in contact with the persons of the experimenters, imparted to them not a scanty, but an abundant portion of its caloric; and that it did so with a rapidity and a readiness far beyond what, in your language already quoted, you seem willing to admit.

In confirmation of the correctness of this interpretation of your meaning, you acknowledge that the air of the rooms in which they experimented, flowed to the persons of the experimenters, in an uninterrupted current, came into contact with them, was immediately cooled, to the extent, in one case, of more than 150° and then, descending their bodies, 'ran along the floor,' in a cool stream or sheet, nobody knows how far, to what point, or for what reason. On neither of these points does either your narrative or philosophy sufficiently inform us.

Though you do not yourself say, that the portion of air, which came into contact with the persons of the experimenters, lost by the act more than 150° of its caloric; yet such was the fact, as the record of the experimenters transmitted to us testifies.

The temperature of the persons of the experimenters was about 100°; while that of the atmosphere at a distance was from 224° to 264°. But when that high-heated atmosphere flowed toward the experimenters, and came into contact with them, it was speedily cooled down to their own temperature of 100°, and, in consequence of its newly acquired weight, descended their persons, as you assert, in a stream or sheet, and thus ran from them, to give place to such other sheets as might succeed it. During this change of place and condition, it was deprived of at least 124° of sensible heat, which, in some way attached to the bodies of the experimenters, with-

out raising their temperature. Here are three or four important facts to be held in remembrance—sensible caloric was withdrawn from the atmosphere, which reduced its temperature—and it attached itself to the bodies of the experimenters, without increasing theirs. Nor was it returned into the atmosphere in a sensible condition. In truth, as will presently appear, it was not returned into the atmosphere at all—but remained insensible and inoperative in the persons of the philosophers.

Thus stands the case, according to your own telling. From what source, then, permit me to inquire, do you derive your authority for alleging, that the atmosphere around them withheld its caloric from the persons of the three philosophers? The record handed down to us gives you no such authority. Nor is it furnished to you by the philosophy of the case, nor by any nor all of the circumstances connected with it.

We are told, in the biography of that distinguished orator, that Sheridan once, sarcastically and with no little effect, charged on an opponent in debate, that for his *wit* he always resorted to his *memory*, and to his *imagination* for his *facts*.

Now though I do not impute to you the practice of a similar expedient, yet, were I to do so, you might find it, in the case before us, no trivial task to free yourself from the charge—very especially from the latter member of it. In the emptiness of your memory, you really seem to have appealed, for facts, to the plenitude of your imagination.

But I am not yet done with your allegation, that atmospherical air, being a bad conductor of caloric, withholds that substance from bodies immersed in it. Notwithstanding what you have said, you will not deny, that the atmosphere of the experiment-room parted from its caloric freely, and supplied it very abundantly to the beef-steaks which it *broiled* in 13 minutes, and to the cold water which it heated sufficiently to cause eggs to be boiled to hardness in it, in 20 minutes. Wherefore then could it not impart the same article, with the same readiness, and to the same amount, to the bodies of the

experimenters? Were both of them lifeless, is not the flesh of a man as receptive of caloric as the flesh of an ox? You will not, I persuade myself, deny that it is. From what cause then did the air fail to impart its caloric to living human flesh, as easily and copiously as to dead ox-flesh? To the ground or rather source of this question your review makes no reference. Yet are you bound to refer to it, and to answer satisfactorily the question itself, or frankly acknowledge that it constitutes an objection fatal to your hypothesis. The dilemma is thus prepared and presented. Nor does any privilege remain to you, save the choice of the horn on which you are to hang. I shall only add, in this place, that the very fact, that the atmosphere of the room raised the mercury in the thermometer, (*through glass*, which is a bad conductor of heat) to the 264°th, on the scale of Fahrenheit, is of itself a conclusive proof of the facility with which it parted from caloric. And it constitutes, to your attempted defence of your hypothesis, another objection, whose subversion is utterly and even hopelessly impracticable.

As you have entirely failed then, to explain, by your first resource, the 'effect' which you acknowledge 'strikes one as so wonderful,' I shall now analyze your second, in order to ascertain whether it is in any measure better fitted to solve the question, and remove the difficulty. It is the process of *evaporation*, which you deem so powerful and efficient, as to be able to control and modify, at pleasure, every form and degree of the heat-producing function. But, if I mistake not greatly, you will find that, in the instance I am now considering, you have far overrated the power and efficiency of that boasted agent.

Antecedently, however, to my special consideration of the process, I feel authorized to observe, as a general and leading remark, that, in the case now before us, evaporation is a two-edged sword, which cuts behind, as well as before—*against* your hypothesis, as effectually as *for* it. Both the beef-steaks that were broiled, and the water, in which the eggs were boiled, received of necessity a high degree of

heat. Yet did they both *very copiously exhale* from their surfaces—more copiously perhaps than any equal extent of surface of the bodies of the experimenters. Wherefore then did not the temperature of the latter substances rise to as great a height, as that of the former? In a mere chemical point of view, exhalation from one surface ought to carry along with it as much caloric, as exhalation from another. Nor will you be likely to deny that it does so. One of the difficulties then which you are called on to encounter, but not the only one, is involved in this question, why did it act differently in the case I am considering? But, stubborn as this difficulty is likely to prove, obstacles to your hypothesis of a more intractable character are yet to be presented to you.

When you shall have scrutinized the subject with the requisite degree of severity and thoroughness, (which from sundry considerations I am satisfied you have never yet done), you will find that, for the production of the effects you exact from it, the labors of your favorite agent *exhalation* must be herculean. And you will further find, that, in providing the materials on which to found your estimate of its power, you will be again compelled to draw on supposition and inventiveness much more largely than on observation and memory. In still plainer and more discouraging language, you will be obliged to rely on the two former sources alone, in as much as the two latter can furnish you with nothing suited to your purpose. For your whole theory is an imaginary fabric.

To begin our scrutiny of this matter, with the eyes of the three experimenters. As far as you are informed on the subject, they were neither burnt, nor in any way annoyed by the hot air in contact with them. You are compelled therefore to believe that they experienced from that agent neither pain nor discomfort. But you have no reason to imagine that they were protected from injury by an augmented exhalation from them; because you are told of the existence of no such augmentation. As a philosopher therefore, and an accurate inquirer after truth, you are forbidden to fabricate or conjecture its existence. Here then was the immunity of an

important, delicate, and susceptible organ without any known increase of evaporation.

You are not told that the air inspired by the experimenters, though it broiled beef-steaks, and boiled eggs, pained or irritated, by its burning temperature, either the lips, mouth, or fauces, the nares, the larynx, the bronchiæ, or any other portion of the respiratory apparatus. Hence you are constrained to believe that no such discomfortable effects were produced by it. Yet no report has given you reason to allege, that exhalation from those organs was in any measure augmented. Whatever therefore may be your secret wish or suspicion on the subject, you will not, I am sure, either discredit your philosophy, or blemish your love of truth, by *asserting* its augmentation.

Nor, when the experimenters assure you, as they do, that, instead of *exhaling* and *gas-forming*, their persons were converted into *condensing* and *water-forming* bodies, can you fail to believe them. For not only were they men of high and well-trained intellects, which qualified them to discern and judge; they were surpassed by none of their contemporaries, in rectitude, and moral standing. Their report therefore must be received as correct and indubitable. And that report is no less than a death-blow to your *exhalation* hypothesis.

The three philosophers tell you, that their persons, by their comparative coolness, attracted from the vapor in the atmosphere around them, so much caloric, as to condense that vapor into water. And they further tell you, that the water thus formed was so abundant, as to settle on their skins, in a statum or sheet, and even to trickle down them in minute streams. To prove to you moreover that this *was water* formed from the atmosphere, and not sweat, they state, as an additional fact, that, on the surface of a Florence flask held in their hands, at the temperature of 120°, the condensed vapor settled in the form of water; and that, when one covering of that fluid was wiped off the glass vessel, by a

napkin, another was soon condensed from the same source, and deposited in its place.

Permit me here to put to you another question. Do you, can you, seriously and sincerely believe, that while water was thus forming and settling on the surfaces of the bodies of the experimenters, by their inhalation of *caloric from without*, perspirable matter was *exhaling* from them, by the action of the *same substance on* them or *within them?* In other words; can the human skin be, at the same moment, and in the same spot, at once an imbibing and condensing, and an eliminating and dissipating surface? Can it, at the same moment, and on the same spot, act in two contrary modes, and in two exactly opposite directions?

We are told, I believe correctly, (indeed you have told us yourself) that, in a humid atmosphere, the evaporation of the perspirable matter is greatly diminished, if not entirely prevented; and that hence arises the oppressive influence exercised on us by air, at once inordinately heated and surcharged with moisture. And, that from the same source arises the same oppressiveness of a long continuance in an over-heated water or vapor bath. If then evaporation be impeded or completely arrested, by a humid atmosphere alone, how can that process be performed, under the combined action and opposition of a moist and heated atmosphere, and a sheet of water on the skin itself? Is it possible for the surface of the body, which condenses into water the atmospherical vapor in contact with it; to secrete itself an aqueous vapor out of the blood, and issue it into the atmosphere? Can it, at the same moment, perform two processes, or forms of action, so immensely different from each other? *Never*—I repeat confidently—NEVER. And, if I mistake not immensely, truth and nature, by way of their sanction, echo the reply.

Individually I am unable, after the strictest examination of the subject, to perceive any ground of belief, that the entire body of man can, at the same time, abstract and imbibe caloric from surrounding atmospheric vapor, and discharge it into the atmosphere by the evaporation of perspirable matter. As

well may you contend that two opposite currents of any other substance, whether ponderable or imponderable, may meet and pass directly through each other, without mutual collision and interruption. Condensation and evaporation are two contrary processes, produced on contrary principles, and by contrary modes of action. Your notion, therefore, that they may coexist on the same surface, is as unphilosophical, not to say preposterous, as that which would contend for the harmonious coexistence, in the same spot, of any other two contraries.

Nor is this all. According to your own view of the matter, a much larger amount of caloric must have entered the bodies of the experimenters from without, than was produced within them, by their own action, and carried away by exhalation from their surfaces. Under these circumstances, unless they had possessed a peculiar power to neutralize caloric, and render it insensible, their temperature must have been increased by it.

But were I even to concede the point, that the evaporation of their perspirable matter did prevent the temperature of the bodies of the experimenters from rising; had that evaporation from the person of *one* of them such an amount of cooling power, as to reduce, in a few minutes, to the extent of 13°, the temperature of the whole atmosphere of a room of no inconsiderable dimensions? To this question you will not hazard an affirmative answer. Were you to do so, the act would indicate in you a want no less of sound science than of sound judgment, to an extent far beyond what I am willing to admit. In truth, I perceive not a single principle or mode of action, on or by which evaporation of perspirable matter from the body of an individual can, to any extent, *or even at all*, cool any portion of the atmosphere around him. To another and very different cause, therefore, must the *frigorific* agency referred to be ascribed.

Nor is that cause either deeply or darkly concealed from an enlightened and unprejudiced inquirer. It is the readiness with which the air of the apartment gave up its caloric, and

communicated it to the persons of the experimenters, who came into contact with it. To no cause other than such surrender of its caloric by the atmosphere, and the communication of it to the body of the individual who entered the room, can the sudden reduction of temperature be ascribed. Nor will you deny that, all things considered, the reduction was great as well as sudden.

The very conception that the exhalation of perspirable matter from the body of the experimenter in the room exerted this frigorific influence, you will excuse me I hope for pronouncing an act of utter *thoughtlessness*, and want of reflection. For offering this remark I have two reasons, either of which is to myself satisfactory.

Had the exhalation existed, it would, as already remarked, have been altogether incompetent to the production of the effect. But that it did not exist, there is strong reason to believe. Let me here be distinctly and perfectly understood. My meaning is, that when the temperature of the atmosphere of the room was at its height (from 224° to 264°) we have no satisfactory reason to believe that the experimenter who subjected himself to its action, was thrown by it into a copious sweat, and exhalation. On the contrary we have very probable ground of belief that he was not.

In the first place, when the temperature of the atmosphere of the room ranged from 100° to 120°, we are given to understand that the experimenter exposed to it perspired and exhaled abundantly. But when the temperature was raised to 200°, and upward, we are informed, not that the person exposed to its influence sweated and exhaled, but that his body condensed into water the vapor already contained in the atmosphere. And had he copiously perspired and exhaled at the same time, the fact would have been recorded, (if for no other reason) on account of its *singularity*—to say nothing of its significance and weight, in the settlement of the question under discussion. Nor is this all.

Though I do not assert it to be a *fact*, yet do I regard it as a *strong probability*, that air of a very high temperature

(say from 150° to 200° and upward of Fahrenheit) acting on the human skin, instead of *promoting* perspiration, actually *prevents it.* Perspiration is a natural and healthy process, to be promoted only by natural and salutary impressions. But, the impression made on the skin, by a very high temperature is *deleterious*—it amounts to *scorching*—or something near to it—under which condition perspiration does not occur. The *vis conservatrix* (a power which unquestionably exists) resorts, for the preservation of the system, to some other mode of action, adapted to the occasion.

Stimulate any organ of the body up to the point of *disagreeable* irritation, which is tantamount to disease, and you greatly impair its natural and healthy function—if you do not for the time, entirely suspend it. Plunge your feet and legs into water so hot that it burns and gives pain—and no perspiration is produced. Scorch your hands or face, by approximating them too closely to a hot fire, and the same will be true, in relation to them. They exhale nothing, but remain dry and rather harsh. The cutaneous vessels become too deeply gorged with blood, and are thereby too much altered in their condition to secrete from the fluid which overdistends them the matter of perspiration.

Nor do I believe the case to be different with the cutaneous vessels, when they are irritated by atmospherical air, at the temperature of 200° and upwards. Their healthy condition is so essentially deranged, that they cannot perform their *natural* function. But that function is the secretion of the perspirable fluid, which, in the case before us, is temporarily suspended—suspended I mean until the removal of irritation and its efficient cause.

In further evidence of the correctness of my view on this subject, take two patients in the acme of the hot-stage of an intermittent—spunge with cold water the person of one of them, allowing the other to remain untouched; and mark the issue. You will find that in the former, not only will the febrile heat be soonest subdued—*the sweating stage will be soonest induced.* The reason is plain. The temperature of the

skin, which had been previously too high, is reduced by the sponging process to the 'sweating point.' For the same reason (the reduction of temperature) venesection, judiciously employed, promotes both *sweating* and *purging;* to the *latter* of which processes secretion is as essential as to the *former.* In these cases you, *as a chemist*, make the effect *antecede* and *control* the cause, instead of following and obeying it. But nature tolerates no such perversion.

Again. At the time of the performance of the experiments by Fordyce, Banks, and Blagden, the question was keenly agitated, whether the immunity of those gentlemen from burning, by the highly heated air, arose from *chemical* or *physiological* influences? And the experimenters themselves, who were well informed in both the branches of science referred to, declared unanimously in favor of the latter. They persistently asserted, that chemistry had no agency in their protection. And, in justice to them, as well as to the cause of truth, I do not hesitate to express my persuasion, that they were fully as well (or rather much better) prepared to judge of and interpret their own experiments, than either you, Liebig, or myself—or than all of us united.

At the period here alluded to ***Black's*** theory of animal heat was as strenuously opposed and defended as is *Liebig's* theory at present. But where is the *former* theory *now?* Precisely where the *latter will* be, before the lapse of the *tithe* of a century---peacefully reposing under the waters of forgetfulness—or remembered only as a signal display of deep error and bold pretension in its author, and of corresponding credulity and delusion in its adopters and advocates.

On the whole, then, as a fair and legitimate inference from the foregoing chain of facts and argument, I feel authorized to say, and do not hesitate to say positively, that one of the two following positions is true:—Either the experiments of Fordyce, Banks, and Blagden, and their extraordinary issues are incorrectly reported, or the living human body possesses a power hitherto unexplained, of protecting itself from the deleterious influence of an overheated atmosphere, by changing

very abundantly *sensible* into *latent* caloric. And let me ask you to bear in mind, and calmly reflect on what I have said, respecting the influence of a very excessive temperature on the human skin—that it seems to *prevent* it from *secreting*, and of course from *exhaling* perspirable matter. If this be true, it settles completely the interpretation of the experiments, and rescues them forever from chemical ambition, usurpation, and perversion. But, to proceed with my remarks.

Your singular comment on the following passage, extracted from my Critique, deserves perhaps a succinct notice.

"Take," I observe, "a large tubful of water heated to the temperature of 120° of Fahrenheit. Immerse your feet and legs in it, and the sense of burning produced by it will be painful to you. Allow your limbs to be still for a few minutes, and the burning will cease. Remove them to another place in the water several inches distant, and the burning will be reproduced. Hold them again motionless, and again you will be freed from pain."

With the position stated in this quotation, you make short work. The following are your words. Of the sentiment you designed to express by them I am not very confident. And the reason of my uncertainty may appear singular to you. The opinion which your language seems best calculated to convey, is so widely apart from what I regard as either *fact* or *probability*, that, notwithstanding the extent of your mistakes elsewhere, I am half inclined to doubt, whether, in the present case, I may not impute to you a notion you did not mean to broach.

"The cause of this" (the aggregate of the phenomena represented in the foregoing extract from my Critique) "is so palpable, that it must have occurred to the mind of the *casual reader*. It is manifestly due to the circulation of the blood in the limb. The blood being 100° reduces the temperature of the water, ('a large tubful!!') and is *itself heated*, at the same time; but as the quantity heated is small in proportion to the mass of that fluid, the general temperature is not sensibly raised in the interval during which the experiment is continu-

ed, and *might not be increased at all;* the perspiratory process and consequent evaporation carrying off the excess of calorific."

Instead of applying to this extraordinary paragraph, with a view to the designation of its character *in words*, any of the condemnatory epithets, which it invites, I shall proceed at once to a brief analysis of it, *show* what it is *in fact*, and leave to others the task of bestowing on it a suitable name. And I feel persuaded, that, in the baptismal ceremony, no predilection for an authoritative appellative will be likely to tempt a judicious sponsor to affix to it, as a *soubriquet*, the name of either Black or Priestley, Lavoisier or Davy.

Permit me then to begin my analysis, by putting to you a few questions, plain, indeed, and unceremonious, but perhaps sufficiently significant and searching.

How do you know that the blood in the limb immersed in warm water 'is itself heated,' and that by that means the water, for some distance around the limb, is cooled? Have you ever either witnessed a decisive experiment on the subject, or made one yourself? Have you, for example, drawn blood from the immersed limb, ascertained its temperature, and compared it with the temperature of the blood extracted from other parts of the body? Or have you taken blood from the same limb before and after immersion, and ascertained the temperature of that drawn at the latter period to be the highest?

If you have performed any or all of these experiments, with such others as may be requisite to settle the question, and have found the blood in the immersed limb to be always the *warmest*, then are you privileged to state your knowledge and experience to that effect; and your testimony will be deemed valid and valuable. But, if you have not thus experimented and thus discovered (and I verily believe you have done neither) on what ground, or for what purpose have you first asserted that the temperature of the blood in the immersed limb is increased, and then, by a semi-condemnatory or self-contradictory equivoque, acknowledge that it 'might

not be increased?'—in other and more significant terms, that you did not know whether it was or not—but that you conjectured and hoped it was; because that issue would best com port with the exigency of your hypothesis.

Having proposed these questions, I shall reply to them myself; and, in so doing, I shall express at least a suspicion that you have asserted the increased temperature of the blood, if not perfectly *at random*, at least without being warranted, by sufficient authority, to speak so confidently to that effect. And I therefore further say, that if such be the case, the assertion was not unexceptionable in you; because it involves a departure from the strict requisitions of philosophy, and the principles and rules of sound discussion. Nor is this all. It indicates a disposition to resort, in cases of difficulty, to assertion, as a substitute for fact. It is in reality a pretension to knowledge which you have not yet attained; and which, without accurate experiments not easily performed, you will find it impossible to attain.

Nor have I yet exhausted the whole store of testimony opposed to your hypothesis. The testimony of analogy, as far as it may avail, is directly against it.

The blood of the three experimenters was not increased in temperature more than one and a half or two degrees, by an atmosphere raised to 264° of Fahrenheit. In truth I am not convinced that it was raised at all. The thermometer, employed to ascertain the temperature of these philosophers, was always, I believe, applied under the tongue, or in the axilla. In either place the heated air of the room had free access to it, and was, if not the entire, at least an auxiliary cause of the elevation of the mercury.

If then a temperature of from 224° to 264°, acting on the whole body, did not augment the heat of the blood, it is highly improbable that a temperature of 120°, acting on but a small portion of the lower extremities, increases the temperature of that fluid. Between your *cause* and your *effect* there is an utter want of proportion—the *latter* being far too large for the *former*.

True; in one case, the application of the caloric is made by water, while, in the other, it is made by air. That however cannot much avail you. The difference between the readiness, with which those two substances part from caloric, cannot be so employed by you, as to account, with any semblance of fairness, for the difference you are anxious to make in the result. It cannot enable you to make it appear, by any form of reasoning, or by anything short of positive experiment, that, while, in one case, a temperature of 264° could not augment the temperature of the blood, in another, a temperature of 120° did augment it.

Once more. Are you convinced, by unexceptionable evidence, that water heated to 120°, increases the temperature even of the skin and the other tissues of the immersed leg, including, of course, the coats of the arteries and veins, all of which are superimposed on the blood, as a protection from the water? Does this question surprise you? and are you prepared to answer it promptly and dogmatically in the affirmative? If so; let me hope that you will excuse me for being somewhat reluctant to concur with you in your answer. *I* am *not* convinced that the skin and the immediately subjacent tissues of the immersed leg *are* increased in temperature, above the temperature of the blood in general.

I do not however contend for the reverse of this. My opinion is suspended, and shall so continue, until it shall have attained a fair claim to be *called an opinion.* And such claim it can derive only from authentic evidence. Without such evidence, what the 'million' *call opinion*, is nothing but conjecture. And, in the present case, that is perhaps the proper appellation for both your (so called) opinion and mine. As relates to the matter at issue, neither of us possesses conclusive evidence.

I am probably myself the author of the experiment in question, as a form of trial, to be employed as evidence, in a philosophical inquiry. And I have never performed it in such a way, as to convince myself of the real temperature of the blood of the immersed limb, and that of the tissues whidh sur-

round and protect it. I do not feel authorized, therefore, to speak of it with positiveness. Much less do I consider you authorized to speak positively of it, who, I am inclined to suspect, have never experimented on the question at all. I feel perfectly satisfied that your attempt at an explanation of the phenomenon has no foundation in fact. The error into which you have fallen is more fully exposed, by the following case.

John Hunter took a *living* and a *dead* penis, at the temperature of 92°, and, immersing each in a separate bowl of water, at the temperature of 118°, the *former* rose to 102½°, and the latter to 114°. And the water containing the living penis was cooled far below that which held the dead one. So great was the difference in this respect, that when the thermometer was placed in the water, by the side of the living penis, the mercury fell to 104°. But, when the instrument was removed from that situation, and immersed in the water, by the side of the dead penis, the mercury rose again to 117°—the water in that bowl having lost but a single degree of its original temperature—while that in the other had lost 14°.

On what ground, except an *occult vital one* will you attempt to explain this striking phenomenon? Pray, do not reply, that the bowlful of water was reduced in temperature 14°, by the small quantity of blood which circulated through the vessels of the living penis—else shall I be compelled to consider you eitheir *in* jest, or *out of* your right mind. Your cause will be again utterly out of proportion to the effect to be explained—I mean that it will be far too feeble for it. And such is also the case with respect to your attempt to account for the cooling of a tubful of hot water, by the immersion in it of the feet and legs. The cause assigned by you I repeat is entirely incompetent to the production of the effect. Nor is there, as far as I can perceive, the slightest conformity of relation between them. And, notwithstanding your allegation, that the explanation assigned by you, for the cooling of hot water, by the immersion of the human feet and legs,

is so palpable, that it must have occurred to the '*casual reader*'—notwithstanding this half-boast, half sneer, on the occasion, permit me to offer you an assurance, that I have not yet met with a single 'reader,' whether 'casual' or regular, who does not reject your explanation—highly 'palpable' and satisfactory as you pronounce it. So striking is the difference between the estimation which writers themselves not unfrequently set on their own thoughts and decisions, and that which is set on them, by the bulk of their readers! I am therefore compelled to adhere to my conception on this topic, heretofore expressed, that the living body of man possesses a capability, not yet explained, of converting *sensible* into *latent* heat, its own temperature experiencing, in the process, but little alteration.

Before concluding this letter, permit me to refer, as briefly as may comport with the intelligibility of my remarks, and in other respects suit my purpose, to an effort you have made to defend Liebig, in one of his positions, in which I consider him, and shall endeavor to prove him utterly indefensible. *His* false doctrine, and *your* inefficient effort to expound and sustain it, are as follows:

"All *vital activity*," (says the Professor of Giessen) "arises from the mutual action of the oxygen of the atmosphere and the elements of food."

The doctrine, or rather notion, here unconditionally proclaimed, is sufficiently extraordinary and startling; yet not more so, than your comment on it.

"All this," you say, "is *intelligible* enough."

And so it is, until you rendered it *unintelligible*, by an effort to elucidate or obscure it (I can hardly tell which) by the following self-inconsistent and self-contradictory expressions. But let it be distinctly understood, that while I admit Professor Liebig's proposition to be '*intelligible*,' I am far from admitting that it is also correct. Far otherwise. In your defence of it, you say:

"Activity results from the chemical changes, but a principle is resident in vital beings other than a chemical power—a peculiar force, as expressed by Liebig, because it exhibits manifestations which are found in no other force. This force moulds and shapes the organism, directs muscular motion, and the secretory processes, and supplies to the oxygen inspired from the air the carbon and hydrogen to be consumed in a slow combustion. This principle extinct, or impaired by an injury to the nerves, the fuel is no longer furnished, and the vital temperature declines."

On this extract from your own pen I offer no comment; because I feel my incompetency to the task. I can neither write nor speak about what I am unable to understand. And such is my case in the present instance. So profound is your matter, so dusky and confused your style and manner, or so dull and unimpressible are my perceptive powers, that your meaning is either beyond my comprehension, or in some other way concealed from it.

Through the thick haze, which appears to me to have enveloped you mind, when you composed the extract, I perceive, or fancy I perceive, the outlines and half-developed images of flagrant error, and, as already intimated, self-inconsistency and self-contradiction. But, in the whole mass, there is neither a feature, nor even a line so distinct and well defined, as to be susceptible, by aught that I can do, of either analysis, description, or comment. I therefore leave it, without further notice, to the consideration of others, who may be better prepared to examine and appreciate it. On the assertion, however, of Liebig, that

"*All vital activity* arises from the mutual action of the oxygen of the atmosphere, and the elements of the food:"

let me ask your attention to a few remarks. And, if I am not mistaken, it will require but a few, and those of the plainest and simplest kind, to make it clearly appear to every intelligent and unprejudiced mind, that the German Professor has, in the preceding passage, fallen into an error as gross and glaring, as man can commit, or language express. In

direct opposition to the time-worn but never-disputed maxim, '*ex nihilo, nihil fit*,'—out of nothing, nothing can be made—he asserts that out of nothing, *something* can be made. He represents an *absence* or *want* to be equal to a *presence* or *possession*—a positive *vacuum*, to an actual *plenum*. To prove this from his own words:

Oxygen is dead matter. So are *carbon*, *hydrogen*, and *nitrogen*, 'the elements of our food.' Mix, combine, and amalgamate those substances *exclusively* with each other, in any way you may please, or can devise, and they form nothing but a mass of matter *essentially* dead.

This position, *in the abstract*, no one—not even Liebig himself, will venture to deny. Yet does that writer, when he connects it with his hypothesis respecting the source of vitality, positively deny it, in the quotation I am considering. By the 'mutual action' of those four *dead* material elements, he openly declares that *living* matter is produced. For, no where, but in *living matter*, can 'vital activity' exist. To deny this, or to assert the reverse of it, would be an act of preposterous folly. Yet does he, I repeat, in express terms, assert, that 'from the mutual action' of those four *dead substances alone*, '*all vital activity*' (that is all power to act *vitally*—or all *vital* power to act—*vital power*, in fact, in every interpretation that can attach to the expression) '*arises*'!! Nor can anything be more *flagrantly contradictory of truth!*

If I am correct in my interpretation of Liebig—if he does assert that, 'from the mutual action' of *dead elements*, 'arises matter possessed of 'vital activity'—and he expressly does so—in such a case it is not possible that you will or can persist in your defence of his false doctrines. Conscience will unite with judgment in impelling you to abandon it, and become the advocate of truth. And, if you perceive me to be incorrect in my version of Liebig's position, I pray you to oblige me, by disclosing to myself privately, or to the world publicly, the mistake I have committed, and I will promptly renounce it. In either case I shall experience sincere gratification. For it is truth and not victory, for which I contend. And now let me close this letter, by asking, once more,

the same question, which, without having received a reply, I have heretofore proposed scores of times to yourself, and other members of the chemico-physiological school:

What is the genuine meaning of the phrases—animal chemistry—vegetable chemistry—or vital chemistry?—for I presume the import of the three forms of expression is the same.

Do the advocates of the doctrines of animal and vegetable chemistry contend, that, in the *formation* of living animal and vegetable tissues and secretions, the forces called *chemical affinities* take part, as positive and efficient agents, in like manner as they do, in the production of various sorts of *dead* matter? In the formation, for example, of a leaf, a blossom, or any sort of fruit; and in the formation of any animal fluid, tissue, or organ,—of the blood, the bile, the urine—a muscle, the eye, the kidney, the liver, the pancreas, the brain, or any other kind of organized and living animal or vegetable matter—is it contended that, in the formation of these products, the chemical forces act as they do, in the formation of granite, marble, silicious crystal, sulphuric acid, sulphate of iron, muriate of soda, acetate of lead, sulphate of magnesia, or any other sort of dead matter? Is it in fact contended that, in the former cases, they positively *act at all*, instead of being *acted on*, by a superior and controlling force? Is it contended that they are *agents* and not *subjects?* If such be the doctrine of the animal and vegetable chemists, I believe them to be as deeply and essentially merged in error, as they would be, did they contend that the brick, the mortar, and the trowel, are *spontaneous* and *positive agents*, in the construction of a wall, instead of being subjects and tools in the hands of the mason—that the paint, the pencil, and the palette are *active of themselves* in the process of painting—or that the chissel and the mallet are *agents* and not *subjects* in the art of sculpture.

In architecture, the brick, the mortar, the trowel, the timber, and the edge-tools all possess their natural properties—

their form, their attraction of gravity, their attraction of cohesion, some of them their elasticity, and several other essential attributes. In the erection of the fabric, however, these attributes are not *agents*, but *subjects* expressly under the control of the builder. Of all the materials and instruments of the sculptor, the painter, and the agriculturist, the same is true. They are *subjects* to be acted *on* and *with*—not *agents* to act of themselves. The builders, the painter, the sculptor, and the agriculturist themselves are the vital and directing principle of action of the whole.

Such is my doctrine. And its applicability to the case I am considering is direct and obvious. In the construction of living organized beings, the constituent materials employed in the process probably part from none of the properties they possessed as *dead matter*. They retain their form, their attraction of gravity, their attraction of cohesion, and their chemical attraction. But these attributes take no active and efficient part, in the formation of the living organized fabric. They are but *passive subjects*, not *operative agents*. They are subject to the control of the vital principle, which bears to them the same relation which the wall-builder does to his brick, mortar, and trowel, the painter to his pencil, paint, and palette, and the sculptor to his mallet and chissel. And as soon shall the paint, brush, and palette, without the guiding hand of the artist, produce a picture, or the chissel and mallet a statue, without that of the sculptor, as oxygen, carbon, hydrogen, and nitrogen a living, organized being, without the agency of the vital principle.

As well therefore may you invent and bestow on *wall-building* a name designed to express the fallacious notion, that the brick, mortar, and trowel *perform the work themselves*, or fabricate for painting an appellation intended to set forth the pretension, that the pencil, paint, and palette can, of themselves, execute a picture, as employ, as you do, the deceptive phrases 'animal chemistry,' vegetable chemistry,' and 'vital chemistry.' You would not, by your new-moulded expressions, mislead the unthinking multitude more essentially,

than you do, by those already in use. By the latter forms of expression, you produce in many, the groundless belief, that *dead* performs the work of *living* matter; and you could not, by the former, effectuate anything more deeply preposterous.

Another analogical view of the matter may perhaps present it in a more impressive and satisfactory light.

Each private of a skilfully enlisted and well-disciplined army possesses, on the field of battle, all the qualities of a thoroughly trained and competent soldier. He is strong and active, hardy, fearless and brave, and perfectly adroit in military movements and the use of his arms. And all these attributes are *elements* essential to the issue of a well-fought and victorious battle. But they cannot *of themselves* achieve that issue. They are but means to be employed in its achievement by a superior agent. And that agent is the commander-in-chief, who is the *vital principle* of the army. Nor, without him, could the soldiers, with all their high qualities, *as soldiers*, any more accomplish victory, than could the chissel and the hammer complete a statue without the directing hand of the sculptor. The commander produces and governs all the tactical manœuvres of the soldiery, precisely as if they were automatons composed of dead matter—precisely as the chess-player moves the pieces of ivory in the game which he plays. And as soon shall the soldiers, *of themselves*, gain a battle, or the chess-men, a game of chess, as 'the oxygen of the atmosphere, and the 'elements of food' construct or support a living animal, without the agency of the vital principle. Nor, though his hypothesis is a *positive denial of it*, will Liebig, or any of his followers, venture, *in words*, to deny this truth.

Finally; I am anxious that my opinion of Liebig be correctly understood. And it is as follows:

In chemistry and some of its applications, I regard him as one of the great lights and leaders of the age. But, in physiology, pathology, and medicine at large, I consider him as directly the reverse—one of the most wild and reckless *speculators* and *misleaders*, not to say *impostors*, of any age.

Such, in relation to the formation and economy of living

organized matter, are the principles and doctrines which I have long adhered to, and endeavored to defend; because I believe them to be founded in truth. And, unless they shall be assailed hereafter, by objections much more powerful than any or all of those that have been hitherto arrayed against them, you are correct in alleging, that I shall not be likely to '*give them up, until I shall have first given up the ghost.*'

With high and sincere regard, I am,

My dear sir,

Very cordially

Yours.

CH. CALDWELL.

June, 1844.

P. S. Though this letter is already considerably longer than I either wished or designed it to be, I must notwithstanding ask the favor of you to indulge me in the addition of a brief postscript.

I have long been, and still am, as already mentioned, exceedingly anxious to receive from you, or some other leading chemico-physiologist, whose opinion may be regarded as *orthodox*, and *authoritative*, a sound definition of '*vital chemistry*,' either *animal* or *vegetable*. My desire is, that the definition may be one so clearly and perfectly intelligible to me, that I may be able to make it the subject of a few remarks, either in approbation or condemnation of it, as my judgment may dictate. And, with a view to facilitate a compliance with my request, I will offer myself, on the *general subject*, two definitions, closely relevant to the end contemplated. A comment on them may, I think, be so moulded by you, as to answer my purpose.

By chemistry I understand that science, which treats of the reciprocal action of the *corpuscles* or *primitive particles* of matter, placed at inappreciable distances from each other. Of this action the solid products are bounded by *right lines* and *angles*, and constitute masses of matter, possessing in

their structure, no fitness for the performance of any given sort of operation, or the production of any specific effect.

By physiology I understand that science, which also treats of the mutual action of corpuscles or minute portions of matter, placed at like distances from each other. But of this action the solid products are widely different from the foregoing ones. Instead of right lines and angles, they are bounded by *curved* lines. And, in form, they are *organs* or *instruments* expressly designed and fitted by nature for the performance of given sorts of action, and the production of certain specific effects. They are structural means expressly prepared for ulterior purposes.

Do me the favor to inform me whether you concur with me in these definitions; and oblige me further by stating, what relation they bear to your definition of 'vital chemistry.' And, provided you agree with me, in my definitions, have the goodness to say further, whether you also agree, in opinion, with Professor Liebig, when he asserts that *physiological processes* are performed on principles 'purely chemical?' For, in his Animal Chemistry, such expressly are several of his assertions.

One objection more to your theory of animal heat, and I am done. And, if I mistake not, you will find it both stubborn and formidable.

After an operation for aneurism on a large and principal artery (say the popliteal) but *very little* blood circulates through the limb, especially the lower portion of it, for *several days*, and but a *moderate quantity*, for *several weeks*. Yet does the limb, cold at first, regain, *in a short time*, its *healthy temperature*. This it does when the amount of blood circulating through it is exceedingly small. But the blood alone conveys to it all the oxygen, hydrogen, and carbon which it receives. Those ingredients in it are, of course, correspondingly small in quantity. It contains therefore but very little *fuel* to be consumed in its '*stove*,' and but very little oxygen to consume it.

From what source then does the limb derive the caloric

which restores its natural temperature? From the combustion of carbon and hydrogen it cannot derive it; because, I say, it is destitute of those combustibles.

When the surgeon applies his ligatures to the diseased artery, suppose he should also tie up the nerves leading to the limb, so as to prevent their action—or, suppose he should *bisect* each of them in the operation; would the natural temperature then be restored to the member in a short time? No, certainly; it would never be restored.

Hence, therefore, do I feel fully justified in asserting, as a conclusion *demonstratively* certain, that the process, which reproduces the temperature of the limb, while blood is wanting to it, and cerebral influence supplied to it through the nerves, is *vital action*, and not *chemical combustion.*

If you can detect any *unsoundness*, in the foregoing remarks, or in the conclusion deduced from them, your exposure of it will be received by me as a favor. For I am laboring honestly in quest of TRUTH. And any one who will effectually aid me, in my enterprise, will be hailed and regarded by me, as an *instructor* and a *benefactor.*

C. C.

ART. II.—*An Essay on the Preparations of Iodine.* By JOHN O'FERRALL, Jr., M.D., of Ohio*.

The deleterious influences which were so frequently observed to arise from the free use of Iodine, led Dr. Coindet to institute a series of experiments by which he might if possible ascertain the therapeutical properties of the drug without its attendant danger. The result of his experiments was the recommendation of the iodureted solution of potassa for in-

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* This Essay was submitted to the Officers of the Medical Institute of Louisville, for the Degree of M.D. in February, 1844.

ternal, and the iodide of potassium for external use. His suggestion however was not received with general approbation until recently; the use made of the latter compound among the profession at large has been as a solvent for free iodine, or when applied externally, the internal use of free iodine has generally been conjoined with it, and *vice versa.*

One set of practitioners administer the iodine by drachms, while the experience of their contemporaries will not warrant them in exceeding the same number of grains per dose. Idiosyncracy added to the varied conditions of the constitutions of individuals at different times, and numerous considerations of a chemical nature, may be the cause of so much diversity of opinion between practitioners and writers.

Though a powerful remedy, it acquires its sway by slow, but certain approaches, not like strychnine, or arsenic which cannot be pushed beyond very limited doses, without at once reminding the practitioner that he is dealing with edge tools. Its action on the animal economy, in a great degree, resembles that of mercury: they both promote the excretory functions, and it is in this way, most probably, that they increase the activity of the assimilating functions; it appears to be more general in its operation on the system than mercury; its action on the uterine system is decided; on the secretory functions of the liver, pancreas, kidneys, and skin it is also quite marked: it thus diminishes abdominal plethora, promoting the action of the eliminative, and through them, of the assimilative functions.

There seems to be two opinions in regard to the manner in which the agency of iodine, chemically combined, is exerted on the human system. Some are of the opinion that iodine, and its compounds all follow essentially the same law of action—that is to say, when iodine is indicated it is a matter of little importance which preparation of the drug is used, while on the other hand, we find it maintained that the compound in question, combines the properties of the substance united to the iodine with those of the iodine itself.

To draw a comparison between the effects of the iodide of potassium and iodine itself is a matter of very considerable difficulty. That they present much resemblance in their operation and effects, is generally admitted, as we may infer from the vague manner in which they are indiscriminately spoken of, the term iodine being applied as much to the one as to the other.

This salt as admitting of larger solution in water, and being less liable to irritate the mucous tissues, is very generally entitled to a preference over iodine in substance, syrup, or tincture, and there is scarcely a disease to which the drug, in a simple state, is applicable, that has not been cured by this compound. Still there is considerable difference in their mode of operation, and that not altogether referable to their relative powers of producing irritation. The iodide appears to be much slower in its action on the system than iodine, and in the hands of some it has failed in several diseases in which the employment of iodine has been attended with eminent success: the want of success was probably owing to the mode in which it was used.

The compound has also proved to be preferable to iodine in some diseases, and the success attending its use greater, as in dropsy and diseases of the kidneys attended with albuminous urine, &c.: other points of difference will be shown in subsequent pages. The bad effects which occasionally arise from the free use of this substance may in a great measure be refered to its adulterations, and the chemical changes which take place in it in the alimentary canal; even most articles of ordinary nutriment may often be suspected of modifying or changing its action. Are we not taught by experience that the same substances which in moderation exert a bland and wholesome influence, may when pushed beyond bounds, produce injury and destroy life?

These and other causes which will be spoken of in another part of this essay may explain the extraordinary discrepancy of opinion which has existed among medical men in reference to the true character of this compound.

In regard to this salt it should be observed that a preternatural sensibility or irritability of the system, a tendency to irregular determination, the presence of internal local congestion, prominent gastric or intestinal derangement, general plethora, febrile symptoms, hemorrhage, diarrhœa, and an inflamed and sensible state of tumors, are decidedly counter-indications to its employment, whether externally or internally. In its action on the animal economy it generally directs its power on tumors and diseased structures, not interfering with the healthy parts of the body.

Some of the effects which are seen to arise from the use of this compound.

To a female who swallowed an ounce and a half of the iodureted solution of the iodide of potassium, the consequences were general uneasiness, nausea, acute pain, and burning heat in the stomach, followed by vomiting, headache and vertigo: recovery took place after the lapse of several days. This case is certainly not sufficient to establish any well defined inference, because the effects of the combination of the iodine with the iodide of potassium cannot be correctly distinguished. Still it has its value, if it is only to show that the combination acts energetically.

Two patients, who, on the same day, took a drachm of the iodide in divided doses, were attacked with nausea, vomiting, colic pains, slight diarrhœa, frequency of the pulse, and general exhaustion—both were affected precisely in the same way.

The accounts which have just been given of its effects would, in the absence of contradictory evidence bring the iodine under Mead's definition of poisons. Devergie considered it as a poison, and stated that the fatal dose was a little more than that of iodine: he estimates the last at fifteen or twenty grains. Doubtless his idea of its virulence was in some degree exaggerated; be this as it may, it serves to increase the perplexity, and render the transition to the opposite side more startling.

Dr. Elliotson, whose enlightened efforts have tended greatly to improve our acquaintance with the proper limits of many remedies, finding that two or three grains of this substance taken daily, as was generally ordered, were ineffectual in certain chronic affections, began in a case of scirrhus uteri to administer it in drachm doses several times per day: the result was relief from pain, &c., unaccompanied by sickness or any other unpleasant feelings.

In several cases of scrofula he gave it in six drachm doses per day, for weeks, without any bad effects. Magendie and others have also used it in as large doses as the above without any deleterious effects.

Dr. Buchanan tells us that the half of an ounce may be given at a dose without producing pain of the stomach, purging, or any hurtful effects.

An ounce or more has been given in Paris by some of the practitioners of that city without bad results. Still we may readily suppose whilst receiving implicitly these statements as true, that they evince the tolerating power of some persons more than the propriety or even safety of administering the iodide in such large doses as a general rule. When we compare these last statements with the accounts which immediately preceded them, it must be owned that they draw largely on our credulity.

The article which was administered in such large doses may have been impure.

In numerous instances where physicians were in the habit of using this salt with the utmost freedom, asserting that no bad consequences had occurred from it, on proper inquiry being made into the composition of the article used by them, it was found to be very impure.

These chemical facts might have been sufficient to explain the mystery, if we had not been assured by respectable authority that means had been taken to purify the drug before using it.

That consequences so different, and sometimes opposite, should arise from the use of the same article is a point on

which the mind is not easily satisfied. From ten to twenty grains dissolved in water or some other proper vehicle, given two or three times a day, is a quantity which if continued for a sufficient time, will prove to be equal to the exigencies of any case in which it is applicable. Dyspeptic symptoms, restlessness, headache, vertigo, and slight salivation, are the most common bad consequences which arise from the long continued use of it. Sometimes it seems to produce constipation, at other times relaxation of the bowels.

But the curative effects proceed in the generality of cases independently of these or any other less important consequences, and persons who are under the full influence of it usually require no particular care except as relates to diet.

Of the much dreaded atrophic influence of this substance on the generative organs, but few cases are on record. Dr. Hill cites one in which both mammæ were absorbed in a patient affected with cancer of the breast; one or two cases of absorption of the testicle are also on record; it sometimes destroys the functions of those organs without causing their absorption. A peculiar kind of coryza, characterized by pain in the nose and forehead, and an increased flow of mucus from the nasal passages, occasionally arises from its use. It is said to augment the flow of blood to the lungs and trachea, causing increased secretion of mucus or hæmoptysis, and in some instances rouses to a state bordering on inflammation.

I am led to believe that such effects very seldom, if ever, occur from the cautious use of the iodide of potassium; perhaps from excessive doses, and peculiarity of constitution such bad results may occasionally arise. Its deleterious action on the nervous system has been already partially referred to: the most common are restlessness, headache, vertigo, muscular tremors, and want of sleep, which generally occur in weak and debilitated persons, and such as have used it for a long time, and in large doses. On the surface of the body this substance produces much less remarkable effects than iodine.

In a number of patients on whom Lugol and others tried the influence of baths containing an ounce or more of this

substance, nothing but temporary itching was produced; by others in the form of ointment it has been found sufficiently active to produce inflammation, ulceration, and the final eradication of tumors; a very considerable degree of smarting is no uncommon circumstance arising from the external use. Dr. Gardner states from personal experience or observation, that its absorption through the cuticle relieved tumors in distant situations, and that the supervention of gastric pain and copious bilious diarrhœa occasionally took place.

Small warty excrescences attended with extreme itching and pain occasionally arise.

From the experiments of Devergn on the lower animals, we are led to conclude that it is a substance which if placed beneath the skin will produce irritation, inflammation, and death, and it may be detected in the various secretory organs of the body of the animal after death. John Davis, M.D. thinks, as an external application for the relief of disease, the iodide of potassium is objectionable, in consequence of the potassium rendering the iodine more acrid, and giving it a tendency to inflame the skin, causing a very considerable degree of smarting when brought into contact with an ulcerated surface.

It frequently causes desquamation of the cuticle; this only becomes observable sometime after its use is discontinued, and the skin has been suffered to become dry. The desquamation of the cuticle more generally arises when the iodurett-ted solution of potassium is used, proving that the degree of energy with which it acts on the skin, is in proportion to the amount of iodine in solution. Dr. Peschier, of Geneva, observes that slight pain of the stomach accompanied by copious bilious discharges, generally presented itself in patients using this compound externally.

The experiments instituted by Lugol in reference to the respective action of the iodide of potassium and the iodurett-ted solutions of potassa in baths, are the most conclusive and unexceptionable of any which have been presented to my consideration. From the results obtained by him we are led to consider the iodide of potassium as a mere neutral ingre-

dients; serving to dissolve the iodine and not otherwise participating in the therapeutical agency of the baths.

The first and second series of experiments made by him with free iodine in the baths leave no room to doubt, that when prepared without being previously dissolved in a solution of the iodide, they contain the iodine partially in suspension, and consequently do not present it to the skin in a state of sufficiently minute division. And the bad effects which followed from the use of the alcoholic tincture of iodine in the baths were more marked than the above. Thus, the fact is established, that it is actually necessary for the iodine to be dissolved in a solution of the iodide in order to secure its equal and complete division in the ordinary quantity of water for a bath, and by this combination its action becomes more equal, and above all it can be thus more accurately graduated. He chose seven patients for his experiments, and the results deducible from the effects produced on them are,

1st. The iodide of potassium has scarcely any action on the skin in the quantity not exceeding three ounces in a bath.

2d. Iodine should be regarded as the active principle.

3d. The proportion of iodine in general should not exceed two or three drachms.

4th. Iodine is not completely soluble in water in its simple state, consequently it is liable to excite great local action.

5th. Previously dissolved in alcohol it does not continue in a state of solution, when mixed with water in bath, thus increasing the liability to local action.

6th. The most certain and least objectionable method of preparing the baths, is the previous solution of the iodine in a solution of the iodide of potassium.

One fact in regard to the external use of the iodide is, that in proportion as the cure progresses its power of producing pain and irritation decreases.

Mr. Pereira states it as his belief, that but one house in London, some years since, sold the pure iodide of potassium. Out of twenty specimens examined by Dr. O'Shaughenessy only two were free from adulteration. The carbonate of potassa is sometimes sold for the iodide. The chloride of potassa is sometimes added for imposition.

An easy method for detecting the carbonate, consists in adding to the suspected article, the tincture of iodine; if the carbonate be present the tincture will lose its color; also by immersing a crystal of the suspected salt in lime water, the presence of the carbonic acid will be indicated by the water becoming milky.

The test for the chloride of sodium consists in adding nitrate of silver to the suspected article; any carbonates, chlorides, and iodides are thrown down; digesting the precipitate in ammonia, you take up only the carbonate and chloride; adding nitric acid to the solution, the carbonate is converted into the nitrate, and the chloride is precipitated.

In scrofula.—The success attending the judicious employment of the iodide of potassium in the various grades of this disorder, is so remarkable, that a disease of the utmost prevalence in this and other countries; one which formerly proved to be very protracted, and difficult in its treatment, and one which was considered by many incurable, has come to be perfectly under the control of the practitioner. I might cite in illustration of its curative power, not only cases of the first and second grades of scrofula, but also those in which that disease existed in the most aggravated form. It has effected cures in extensive tubercles of years' standing, of strumous ophthalmia, coryza, extensive ulceration of the cellular system, also of induration and hypertrophy of the same, extensive disease and ulceration of the cutaneous system, white swelling of the knee, hips, shoulder, elbow, &c., attended with caries of the bone.

In bronchocele.—Dr. Coindet seems to have been the first person who used the iodide with success, since which time its curative power over this disease has been well establish-

ed; still the proofs of this power, for reasons already assigned, are not so numerous as are those of the power of iodine. He was so well satisfied with the results obtained from applying it externally as to be fully warranted in recommending this as the only unexceptionable method of using iodine. Many do not agree with him in consequence of finding such a plan by itself not sufficiently active, and they combine the internal use of it with the local.

A great number of cases illustrative of its specific power over this disease might be quoted from authors, several of whom deemed it scarcely necessary to pay any attention to other remedies which were formerly recommended for the cure of this affection, and it can hardly be presumed, that where the judicious employment of the iodide fails, there can be any particular advantage derived from any of the remedies at present known.

It has not proved to be equally efficacious in all cases of scrofula; no plausible reason for this difference in its action has been assigned.

In phthisis pulmonalis.—The beneficial effect of the iodide of potassium in scrofula affecting the external parts, induced the hope that it might also prove useful in consumption.

In some constitutions it does not appear to produce any obvious effect either for the better or worse; but in the majority of cases, even in the second stage, its curative operation is quite obvious. The strongest testimony that I have met with to its utility in phthisis is given by Dr. Morton. He states, that having used it extensively, he is able to express an uneqivocal opinion respecting its power. In a large number of instances it has appeared, especially in the incipient stage, to arrest or suspend the tubercular secretion, and with it the hectic, marasmus, cough, dyspnœa, and other urgent symptoms. It also often improves the complexion, and restores the appetite, even when there is no hope of recovery, owing to the advanced stage of the disease.

It so obviously improves the nutritive functions in some cases, that patients have increased in flesh, strength, and spir-

its, by its use, and at the same time recovered to a considerable degree a natural florid complexion. The results of Dr. Bardsley's observations on the use of this drug in this disease are very different from the last.

He employed this medicine in fifteen well marked cases of incipient phthisis, with a strict attention to its effects; in five instances it appeared to arrest the further progress of the disease, but the apparent amendment was only temporary; the disease finally proving fatal.

Dr. Baron says, that he has reason to believe that tuberculous disease of the lungs in the incipient stage has been removed by the judicious use of this compound. Mr. Cooper relates three cases in which marked improvement was perceived in a few weeks after commencing the use of the iodide, and an effectual cure in a few months; the hereditary predisposition to this disease was well marked in all the cases reported by him. I have tried this salt in three well marked cases of incipient phthisis, and with decided benefit in two of them, the cough, dyspnœa, night-sweats, pain in side, &c., having been relieved in the course of a few weeks, and not having since returned. In the other case it seemed to hasten the suppuration of the tubercles, and consequently the death of the patient.

The use of the iodide was continued for some time after recovery in the cases to which I refer. In the second of the three in which this solution proved of advantage, there had been for some time previous to his applying to me a very considerable quantity of purulent matter expectorated. His recovery perhaps may have been owing to his strength of constitution, the predisposition to the disease in him not being very marked.

The other one in which its curative effects were more decided, had suffered several attacks of hæmoptysis, and was very weak, which protracted the cure. He is now following his occupation as a farmer, and enjoying fine health, without the least indication of the further progress of his much dreaded malady. In this case the predisposition was

very strong. It has been six months since he was discharged as cured.

The tubercular disease of the lungs is only a secondary affection, the consequence of a pre-existing morbid state of the constitution, generally a hereditary one, sometimes acquired by various causes independent of a hereditary predisposition; and he who is acquainted with the morbid anatomy of it cannot for a moment indulge the hope of ever being able to cure confirmed consumption, if we except the small number in which the deposit is confined to a very limited portion of the lung. A hope may be entertained of diminishing the hereditary predisposition, by investigating the circumstances in the health of the patients which predispose their offsprings to the affection.

Childhood and youth seem to be the periods of life in which tuberculous complaints are most rife, and fortunately they are those in which much may be done to destroy the tendency to disease. Judging from the effect this substance has had on the few adults, on which it has been used for the cure of phthisis, we may conclude that judiciously emplyed in the young who have a predisposition to consumption, it may be found a remedy of great value in preventing the secretion of the morbid matter, more especially if used at that period in which the tuberculous cachexia exists. In many cases, no doubt, the result will be fatal in spite of all human aid; still there are many in which the progress of the disease may be suspended, and life prolonged for a considerable time; and in a few where the hereditary or acquired predisposition to the deposit of tuberculous matter is slight, it may effect a cure.

Secondary and tertiary syphilis.—Of all diseases constitutional syphilis is perhaps the one which most decidedly requires the prompt interference of art. The first symptoms which make their appearance after chancre should be promptly treated, for the serious nature of syphilis depends in a great degree on the time allowed it to undermine the system.

The iodide of potassium has been substituted by some physicians for mercury, in the treatment of syphilis in all its forms. It is also said that only two cases out of every ten of chancre are followed by secondary symptoms if left to nature. If such is the fact, it proves that eight out of every ten persons with chancre in common practice, are mercurialized without any necessity. This is a very great evil to the scrofulous, and consumptive, and as mania, erysipelas, palsy enteritis, &c., occasionally arise from its use, we should not employ it more frequently than is actually necessary.

Eleven cases of tertiary syphilis are reported by Dr. Bullock, in which this compound effected a perfect cure in a limited time. Dr. Wallace used it in one hundred and forty cases, and with what degree of success I have not been able to ascertain, but judging from the manner in which he speaks of it, it was of no ordinary kind. Professor Gross and others consider this substance as much a specific for this disease as quinine is in intermittent fevers.

Among the various causes which now and then produce looseness of the teeth, none is more common than inflammation of the alveolar processes and sockets. Sometimes this originates in a disease of the teeth themselves or of the gums, but in other instances the diseased action commences in the alveolar processes, and by spreading to the socket and gums it gives rise to great pain, swelling, and sponginess of the gums, and eventually involves the fangs of the teeth, and detaches therefrom the grasp of the sockets and they fall out. The progress of the disease is accompanied by extreme pain, and a purulent discharge oozes out from between the teeth. In several such cases the iodide has been used with entire satisfaction, effecting a cure in a very limited time.

In gonorrhœa the iodide of potassium has been used in quite a number of cases, most generally with success. It has proved more especially beneficial in the chronic variety, many cases of which might be quoted which were cured by it.

In chronic laryngitis, bronchitis, tonsilitis, &c., it is a remedy of the highest value; many cases of each might be cited in which its use was followed by success.

In amenorrhœa this salt has, in the hands of several physicians, proved a remedy of very considerable value, and been successful in most cases in which it was used.

Several young women to whom Dr. Bardsley gave the iodide did not menstruate until their sixteenth year—he thought that process was retarded in consequence of the use of this medicine. The opposite effect has been observed. Dr. Wallace gives one instance in which a female, who had not menstruated for several years, became robust, and menstruated after using this compound for a short time.

John Bell, M.D., says "as an emmenagogue I can certify to its powers." He has also found its administration for a short period, followed by such an amended condition of the constitution in married women, who had been before its use childless, that they soon conceived, and had healthy children. It has been successfully emplyed in chronic diseases of the uterus, attended with induration and enlargement.

In dysmenorrhœa its success has also been very flattering. In leuchorrhœa this compound has exerted a curative power of the most striking nature. Several cases which had proved to be unmanageable by the use of the remedies commonly resorted to, were cured by it in a few weeks.

Chronic enlargements of the liver, spleen, pancreas, prostate gland, &c., are very often materially relieved, if not cured, by the use of the iodide.

In rheumatism, more especially in the chronic stage of the disease, it is one of the most powerful remedies we possess, and is considered by many of our most respectable practitioners and authors as a specific.

Dr. Elliotson, who seems to have been the first who used it in this disease, administered it in doses of from five to eight grains, three times a day, with results highly flattering. I might, if it were deemed advisable, give the particulars of several very interesting cases, which have lately fallen under

my care, in which this compound relieved effectually all the symptoms in a very short time. In these cases it was given in doses of from ten to twenty grains three times a day, according to the effects produced. In several cases in which the disease was merely local, the ointment was used in connexion with the internal administration of the remedy.

In dropsies, especially such as are attendant on, or caused by enlargement or chronic inflammation of the liver or spleen, it has been found to be of very material benefit. Cumming, Bain, Ricord, and others report cases of the different varieties of dropsy cured by it. Dr. John Bell speaks very highly of the iodide in a variety of dropsies, attended with disease of the kidneys, in which albuminous urine is secreted.

A case of anasarca which fell under my observation lately, in which squills, digitalis, purgatives, &c., had been used with very little or no apparent benefit, was effectually cured in two weeks by giving ten grains of the compound three times a day. The patient was salivated when we commenced the use of the substance, but the ptyalism was perfectly relieved in a few days.

In a case of chronic diarrhœa in which the attending physician had tried all the remedies commonly resorted to without the least benefit, the iodide in five grain doses, three times a day, effected a cure in a few weeks. This case was of long standing, following a very severe attack of fever, for which the patient had been salivated.

This compound, according to the results obtained by Dr. Bardsley, seems to have less influence over nervous diseases than iodine. His want of success in these affections may have been owing to the smallness of the doses which he used. for Dr. Manson and others found it to be eminently beneficial in all the cases in which they used it.

That it exerts a powerful influence on the nervous system I am well assured from observation. It has produced in some instances a feverish excitement, headache, wakefulness, giddiness, &c.

In several cases of dyspepsia of very long standing, for the cure of which all the remedies which had from experience been found to be most useful were ineffectually tried, the iodide was finally used, and with entire success.

The curative effects of a strong solution of this substance and iodine can only be appreciated by those who have received benefit from it, two or three applications being generally all that is needed to relieve the pain, &c., of inflammation. It has been used with success in inflammation of the absorbents, in chilblains, malignant ulcers of the tonsils, and tongue, whitlow, burns, scalds, &c.

Lastly, it promises the power of causing determination of diseased action to the skin. In cases of suppressed measles and scarlatina, it will frequently induce healthy reaction under the most desperate circumstances.

February, 1844.

ART. III.—*A Case of Extensive Emphysema, produced by a wound of the Trachea.* By J. G. GIRDNER, of Galena, Illinois.

Henry Foley, a boy aged 5 years, September 2d, was standing on a pile of barrel headings with a pocket knife in his hand, when the piece on which he stood slipped backwards and threw him on his face, and on the point of the knife—the knife penetrating the neck immediately above the left clavicle, about six lines from its junction with the sternum, and passing backwards and rather downwards wounding the trachea. The external wound was nine or ten lines in length—the blade of the knife was about two inches in length and three-fourths of an inch in width tapering to a point. I saw the boy a few minutes after the accident and found him nearly in the following situation. Emphysema over the whole body; even the head and face swelled until

the eyes were closed; great difficulty of breathing, coughing violently and expectorating blood in considerable quantities; air and blood escaped by the wound at every breath, extremities cold, pulse quick and very weak. I examined the spot where he fell and found that he had bled very freely at first—but the bleeding had nearly ceased when I saw him. At the time of the accident he had hooping cough, and was, I think, in the third week of the disease. After applying a piece of adhesive plaster to the wound, and ordering stimulants to the extremities, I waited a few minutes for the violence of the cough to subside, and then proceeded to dress the wound. After sponging the blood away I closed the wound with one interrupted suture, passing the needle through the skin and cellular substance only; strips of plaster, dry lint, a compress bandage, then completed the dressing I saw him again the same day in the evening, when I found that reaction was established, the skin having become warm and moist, and the pulse full. I ordered him to be kept as quiet as possible, and enjoined a very abstemious diet.

3d. This morning I found him comfortable, with a pulse at 108; cough had been troublesome through the night, but otherwise he had rested well. The physical signs differed nothing from hooping cough; a little air still escaped from the wound during the paroxysms of coughing. The expectoration was still streaked with blood.

4th. I found him with rather more fever, for which I ordered sulphate of magnesia, which operated through the day, and gave him some relief. No air has escaped from the wound since 12 o'clock last night. Emphysema spreading to the extremities.

5th. This morning I found him much the same. No more blood expectorated; appetite good. Ordered low diet, and the patient to be kept quiet as usual.

6th. Found him still improving, the dressings still in their places, and that he was with difficulty kept in bed.

7th. To-day he has no fever, and the cough is less trouble-

some. Mucous rhonchus is sometimes heard in one part of the chest, and sometimes in another.

8th. Found him quite well, and the dressings having become loose I cut the stitches and removed it, when I found the wound entirely healed, except a small scab where the thread had been. I passed by the house in the evening and saw him at the cooper's shop, a distance of 30 yards from the house; when he saw me, fearing lest I should scold, he ran to the house as nimbly as usual. The emphysema had not entirely disappeared, but was disappearing slowly, and is now quite gone. The hooping cough has nearly left him; it has been much less troublesome since his wound healed, than it has been to his little brother and sister taken at the same time with him. Whether the abatement of the hooping cough was owing to the loss of blood at the time of the accident, or to the low diet and purgative afterward, or both, I will leave to older and better pathologists than myself to decide. The boy is now entirely well, and the wound in his neck was completely healed in seven days from the time of its reception.

Dr. Buck, of Louisville, who saw this case on the fifth day of the accident found that upon pressure over the scalp, the face, the chest, the abdomen, and extremities, the crepitation was as distinct as in a healthy lung; and remarked that so far as his observation extended, the case was entirely unique as to the extent of the emphysema.

Galena, Sept. 27, 1844.

SELECTIONS

FROM

AMERICAN AND FOREIGN JOURNALS.

Progress of Surgery in China.—Our very distinguished countryman, the Rev. Dr. Parker, Missionary Surgeon at Wampoa, has four pupils who are studying both the English language and medicine. Kwan Taon, the eldest of them, has operated successfully for cataract, upon between twenty and thirty persons; and in one instance, extirpated a tumor from a woman's shoulder, weighing, says the Missionary Herald, about a pound and a half. The patient, treated wholly by Kwan Taon, was discharged well in ten days. This young operator is represented as commanding much respect among his countrymen, and is moreover esteemed, by all who know him, for his correct and gentlemanly deportment. He promises to be a useful man and a blessing to his country. It is worth remarking that this native surgeon professes to have a higher aim than that of simply obtaining wealth—the great and prominent desire of all Chinese, in whatever capacity they are found. When he shall be qualified, according to a prescribed standard, it is presumed, of Dr. Parker's, it will be his choice, he says, to extend the benefits of the knowledge he is acquiring of foreign surgery and medicine, to other cities and other provinces of the celestial empire.

Two thousand one hundred and nine patients were admitted into the Missionary Hospital from July, 1843, to January 1, 1844. Cases of unsurpassed interest are continually presenting, and the treatment continues to be equally successful. The institution is constantly gaining upon the confidence of the Chinese of all ranks and conditions.

Yu, the late Kwang Chowfoo, being about to visit the Emperor, submitted to the removal of a tumor behind the ear,

with the knife. He subsequently went to Dr. Parker's house to have the wound dressed, and once accepted an invitation to breakfast. He expressed his opinions with great freedom, discovering, by his conversation, a mind much in advance of his countrymen generally. The Imperial High Commissioner, Ke Ying, also availed himself of the benefits of the Missionary Hospital. On the occasion of the American Consul's presenting his credentials, at an interview with their Excelleneies the Commissioners, Ke Ying consulted Dr. Parker in person, having done so previously by proxy.

Boston Med. and Surg. Jour.

On the Cure of Hydrocele by Iodine Injections. By Professor Velpeau.—There are few surgical operations so easy, so simple, and which need less preparation; in what, in fact, does it consist? Of a puncture, and the injection of a certain quantity of liquid; after which the patient may be abandoned to nature, which, in general, is sufficient to effect a cure. I have operated on upwards of 300 cases of hydrocele; in my wards at the Charité I receive, on an average, from 30 to 40 patients yearly, and operate, at least, on 10 in my private practice annually; and I now declare that I never had a case in which the consecutive accidents could be attributed to the iodine injection. A patient, it is true, died, after having been operated on; but death here was caused by inflammation and purulent infiltration of the cellular tissue of the pelvis, *without communication with the tunica vaginalis.* It is, therefore, impossible to attribute this to the operation for hydrocele; nay, what is more extraordinary, especially when compared with similar cases in which the vinous injection was made, is that, on one occasion, the liquid, instead of entering the cyst, was injected in the cellular tissue, and yet did not produce sphacelus. The patients are cured by this method, on an average, in a fortnight, and I never witnessed a case of gangrene or suppuration from its use. It is therefore evident, that the iodine injection produces a degree of irritation sufficient to cure the hydrocele, but does not produce a suppurative inflammation; and this has been proved by experiments on animals, where the liquid is absorbed without causing any accidents. The same result has been obtained in cysts situated in other parts of the body; thus, about three weeks ago, I injected one on the neck, and the swelling diminished in size. It must not be supposed, however, that I assert that the iodine

injection never fails; but what may be affirmed without fear of contradiction, is, that it almost always succeeds, failing only once in sixty, or even one hundred cases; unfortunately for mankind, we have but few remedies so certain in their action. This, perhaps, may be attributed to the formula I have adopted. To resume, therefore: the advantages of this method are: 1st, that the cure is almost always obtained; 2d, that the gangrene does not take place, even when the injection enters the cellular tissue; 3d, that the operation is sooner done than that in which the vinous injection is employed, since it is not necessary to heat the liquid, or to leave it a certain time in the cyst, &c. If, after the operation, you wish to hasten the resolution, the patient must be treated as for orchitis, and since scarifications with a lancet are useful in the latter, they will equally be so in the phlegmasia which follows the injection. In the generality of cases, this, however, is not requisite, the patient recovering without any remedy being employed; it, perhaps, takes eight or ten days before he is quite well, but this is not of much importance, as his sufferings are slight. Finally, I have remarked that the iodine injection is a powerful discutient when hydrocele is complicated with *engorgement* of the testicle or of the epididymis.—*London Med. Times.*

The formula employed by Professor Velpeau is the following: ℞. Tinct. iodin. ʒj. Aque distillatæ, ʒij. M.—*Boston Med. and Surg. Jour.*, July 22, 1844.

Fascination.—The late Lord —— had a daughter, who falling ill at Bath, was attended by the late Mr. ——. Of course he soon persuaded her that she had a stricture of the rectum. The young lady wrote to her mama in town an account of her case, and of Mr. ——'s procedure to cure her. Mama communicated this to papa, who not liking the thing much, went straight to an eminent practitioner, who showed his Lordship the great improbability of his daughter being affected in the manner indicated; and made him aware that Mr. ——'s patients were all—men or woman, matron or maid—presumed to be affected with stricture of the rectum, and treated accordingly. His Lordship was in a great fume; he would not even write; he posted off the same evening to rescue his child from the malapraxis, as he believed it, of Mr. ——. But what ensued? The London surgeon received a letter in a couple of days from Lord ——, to say that his

Lordship felt his visit to have been quite a providential one; that Mr. —— had not only satisfied him that his daughter had a stricture of the rectum, but that he himself had one, and that he meant to remain at Bath to have it cured!

Med. Gaz., from *Boston Med. and Surg. Jour.*

Quarterly table of Mortality in Great Britain.—The deaths registered in the last quarter (ending June 30) amounted to 30.925, which is less by 7116 than the deaths of the previous quarter, and 1283 less than the average of the corresponding spring quarter in the five years 1838–42. Allowing for the increase of the town population, the mortality was ten per cent. below the average of the season.

The reduction in the mortality has been unequally distributed over the kingdom; but it has been most remarkable in the large manufacturing districts. Small-pox and scarlatina have been epidemic in the metropolis and elsewhere. The deaths in the quarter from small-pox were 425. The epidemics most frequently mentioned in the country districts are scarlatina, measles, small-pox, hooping cough, and typhus.

Ibid from *Ibid.*

Influence of a Varying State of the Price of Provisions on General Disease and Mortality.—M. Mélier investigates with philosophic spirit this important medico-political question. The statistical documents he has collected, and the tables into which his figures are thrown, bear out the following inferences:

1. The mortality of a country is influenced by the price of corn and bread.
2. This influence was extremely marked formerly, and is less so at present.
3. The diminution of this influence has been gradual, and various causes have contributed to this result.
4. The cultivation of the potatoe is one of the chief of these.
5. The question is one of morality as well as of hygiene, for it is demonstrated that crime increases with the dearness of provisions.

It is inferable too, the author considers, from his inquiries, that, in a well-organized state of society, provisions tend con-

stantly to increase in abundance; this tendency is more marked than that of the population to increase—'a powerful argument against the theory of Malthus.'

British and Foreign Med. Review.

The Local Pathology of Neuralgia has been explained by Dr. Black upon anatomical principles. He very justly observes, that the nerves, which are usually the seat of neuralgic pains, are those which take their exit from the interior of the body through canals in bone or unyielding tendinous structure. He adds to this, the anatomical fact, that each nervous twig is accompanied by a branch of an artery and a vein. It may easily, therefore, be conceived that those nerves, which are contained in rigid canals, must be subjected to injurious pressure whenever their accompanying vessels are unusually distended with blood. Upon this pressure, according to Dr. Wallis, depends the neuralgic paroxysm. The explanation is, I think, borne out by the consideration both of the exciting cause and the effects of treatment.

Dr. Ranking in *Ibid.*

Flourine.—Drs. Will and Fresenius have detected flourine in the ashes of plants—it exists in all the cereals. It has been found in the bones of all recent animals thus far examined, also in fosil bones.—*Ibid.*

Extirpation of the Spleen.—Dr. Berthet communicated to the Academy of Medicine on the 9th of July, the case of a man who in a quarrel was wounded in the left hypocondrium. Eight days after, when Dr. B. was called to the patient, he found a large tumour formed by the spleen, which was gangrenous; he removed it, and by an appropriate treatment, cured his patient, who lived 13½ years after the accident. Digestion took place as usual, and the individual enjoyed good health. The man ultimately died of pneumonia; at the autopsy, the remains of the spleen were found to be no larger than a filbert, and were adherent to the stomach.

Med. Times, July 20.

Calomel as a Remedy in Diseases of the Eye.—On the recommendation of Dr. Major, of Lausanne, Dr. Fricke frequently employs calomel, in the form of powder, with surprising success, where other remedies have failed to produce any effect. The diseases in which he uses it are chiefly: scrofulous rheumatic, and catarrhal opthalmia, particularly when combined with great structural alteration of the conjunctiva and iris, or with effusion into the anterior chamber, and deposition on the cornea. Violent inflammation arose from its use in but two patients, who had formerly taken iodide of potassium for some syphilitic affection. The calomel turned of a yellowish green color in these cases, and this led Dr. Fricke to believe that the iodide of potassium, contained in the lachrymal secretion, combined with the mercury, so as to form a violent stimulant. The author used calomel in all the cases recommended by Fricke, and never observed any other than good effects. He generally blows the powder into the eye by means of a quill, and considers this method less irritating to the organ than the usual manner. He generally takes equal parts of calomel and fine sugar. As to the question, whether its action is mechanical or dynamic—we must certainly decide in favor of the latter; otherwise, every indifferent powder would have the same effect.

Dr. Neuber, from *Schmidts Jahrbucher:* from *Ibid.*

Therapeutic Agency of Mesmerism.—Mention is made in a New Orleans paper of a young lady who, during the mesmeric sleep, had a tooth extracted, without being conscious of any pain. The fact was very astonishing, till it was subsequently ascertained that she had had *sixty-two extracted* before public audiences, to illustrate the wonderful effects of the new science!

Boston Medical and Surgical Journal.

Fresh Vaccine Lymph.—M. Majendie announced to the Academy of Sciences, May 27th, that he had met with the cow-pox in a cow which belonged to him. Dr. Fiard has vaccinated many infants with it, and genuine vaccine pustules were developed. M. Majendie placed the cow at the disposal of the Academy.—*Ibid.*

Spontaneous cure of Phthisis.—Dr, Clarkson, in the New York Journal of Medicine, reports the following case of spontaneous cure of consumption:

It is my belief that there are many more cases of phthis pulmonalis cured by nature alone than medical men are generally aware of. I do not mean to say that we should always trust to nature alone, by any means, but that we should be very cautious lest we do too much. The case that I am about to relate came under my care the 29th day of March last. Sarah Hamor, æt. 55, born in England, had always been rather delicate. She was attacked last fall with erysipelas of the left hand and arm; and after recovering from that, she caught a severe cold, which was followed by a troublesome cough, and shooting pains through the chest, but more especially on the right side, from the mamma to the shoulder blade of the same side. She now began to lose flesh; and when I first saw her (which was as above stated) she was so much emaciated, to use a common phrase, that she was a mere skeleton. She was so weak, at that time, that she was unable to help herself; she had profuse night sweats, severe cough, and expectorated about half a pint or more of light greyish yellow pus of a frothy nature (same as usually seen in the last stages of consumption) in the course of twenty-four hours. Her shoulders were raised and brought forward; the chest flat, and the clavicles very prominent, leaving a hollow. On auscultation there was that peculiar hollow gurgling sound, at times, heard over the right lung; and, at others, there was no sound to be heard of respiration at some points. The dyspnœa sometimes became so urgent that the friends several times thought her dying. Almost as soon as she fell asleep, she would break out in a profuse perspiration, and the little sleep she did get was not refreshing. There was an abscess on the right side of the spine, extending from about the second rib to the ninth or tenth, and about four inches wide, involving all the muscles of the back contained within that space. There was an opening about an inch and a half from the spine, over the region between the fifth and sixth ribs, which I enlarged by means of a bistoury, and kept open by the use of poultices. The abscess discharged constantly, and at one time nearly a pint of well marked pus. When I was first called, I told the friends that I thought she could live but a very short time, which conclusion, I was informed, all parties concerned had come to before having her brought into town

She had been brought over the river from Wallabout, L. I., the day that I first saw her—it being her desire to be at her friend's house, in this city, when her spirit should take its flight. My great object in her case was to palliate her sufferings, and merely make her as comfortable as possible during the time she might live. I told her friends to let her eat as much as she wanted, and anything in the way of ordinary food; also to let her have a small quantity of wine. Ordered the following mixture to be given at night, and at any time during the day when the cough was most troublesome: R. Gum Acacia, Ext. Glycyrrhiz. ää ʒj., Syr. Althea. Tinct. Opii. ää ʒ ss., Vin. Antimo. gtt. x. and Aqua pura 3 iii. Dose, cochlea magn. I visited my patient daily, expecting every visit to be the last, for about a fortnight, when I was convinced that she was no worse: her night sweats began to to decrease, and she got more refreshing sleep; her appetite seemed very good, and in fact was rather voracious, and her cough was easier. The discharge from her back began to diminish, and at the end of four weeks was quite healed up. Her cough had nearly left her at that time, and she began to gain flesh. In about four weeks from the time I first saw her, cough had entirely left her, and she was gaining strength fast. The only thing that she now complained of was pain about the right side, and a "falling in" as she said, of the right side of the chest, which she had observed during the whole course of her treatment. The right side became much smaller than the left, owing to the chest's shaping itself to the new condition of the lung. In about six weeks from the time I first saw her she left town for Poughkeepsie, in better health, she said, than she had enjoyed for years. I gave her nothing of any importance beside cough mixtures, similar to the one first ordered.

There is something very singular in this case, and some may be led to believe that there were no tubercles in the lungs. the affirmative of which I have every reason to believe, for I never saw a plainer marked case. The left lung seemed, from the first, not to be diseased.

Case of Amenorrhœa cured by Electro-Galvanism.—U. W., æt. 35, unmarried, rather delicate constitution; her menses had stopped by taking cold, some seven months before I first saw her. I tried all the usual remedies prescribed in such cases, but Idid not succeed in bringing about the desired effect. I had been using the Electro-Magnetic machine in some other cases with the happiest effects; and I therefore was induced to use it in this case, in which it suc-

ceeded most perfectly. I applied the buttons connected with the machine over the region of the womb, holding one on the lumbar region of the spine, and the other in front over the pubic region; using the negative and positive wires alternately to the spine and abdomen. I continued the remedy five days, from five to ten minutes each day, when her menses were fully re-established. I have used it in rheumatism with good effect.—*Ibid.*

Comparative value of the different preparations of Mercury and Iodine, and the best modes of administering them. By Edward Octavius Hocken, M.D., &c., Physician to the Blenheim-street Infirmary.—Mercury is employed locally and generally, either to produce a local effect simply, or, by its admission into the sestem, to bring the whole constitution under its influence. The mercurial influence is induced in the system by the introduction of mercurial preparations into the stomach, or by fumigation, or by inunction. In the first method we employ the chloride, bichloride, iodide, *pil. hydrarg.*, *&c.*, *&c.*

Chloride.—Calomel is chiefly useful when we wish to produce a speedy and powerful action on the constitution as in venereal iritis or orchitis, but is less adapted to the ordinary symptoms. On the ontinent it is extensively employed in tubercles of the labia, with or without ulceration, in various forms of creeping ulcers, and also in ulcerations of the throat and nasal fossæ. Desruelles says, that he cannot too much recommend this preparation, which united to opium and an antiphlogistic regimen, may produce the most beneficial results. Ricord employs the following pills in the treatment of enlarged testicle, which remains after inflammation of that organ.

Hyd. Chor. ℈i. Pulv. Conii, Sapon. Hisp. aa ℈ij. M. ft. pil. xxiv.

Bichloride.—M. Dupuytren ordered this remedy in small doses, one-sixth of a grain three times a day, in constitutional syphilis, and on the Continent it still continues to be extensively used for this purpose. In some chronic cases of of syphilitic skin disease, I have seen it used with advantage; but as a general remedy in secondary syphilis it requires more care, is more dangerous, and altogether is a less eligible medicine than the blue pill.

Pilula Hydrargyri.—This medicine is the form most used

and relied on in England, and as it is one of the mildest, safest, most certain, and most manageable preparations of mercury, it justly deserves the preference given to it. In doses of five grains two or three times a day, it is applicable to nearly all those conditions which we have shown to be benefitted by mercury.

Proto-ioduret.—MM. Cullerier, Biett, Ricord and others employ this remedy in many forms of constitutional syphilis, especially where secondary and tertiary symptoms are combined, and in primary sores in strumous habits. Cullerier says, that it is chiefly in constitutional syphilis that the proto-ioduret of mercury is administered with success. Its effects are principally evident in secondary ulcerations of the mucous membrane, cutaneous tubercles, exostoses, and chronic affections of the joints, where the other preparations of mercury have had little effect. It should always be guarded by opium, and given in half grain doses twice or thrice a day. The deuto-ioduret is more stimulating, and consequently its dose is smaller. Either of these may be employed in friction upon tumours and idolent buboes, after the removal of all acute inflammatory symptoms.

The cyanuret and deuto-phosphate of mercury are occasionally employed. The former is said to be preferable to the bichloride, being less apt to disagree, and less readily decomposed. It is an useful external application in some skin affections, allaying the violent itching and irritation of what M. Alibert terms *herpes squamosus.*

Inunction.—Inunction by the mercurial ointment was formerly employed to mercurialize the system more frequently than at the present day. In this way the mineral is less apt to disagree with the system, especially the alimentary canal, although, when used alone, it is less speedy in its effects. In buboes, I imagine that Hunter was correct in his opinion concerning the advantages of making mercury pass through the affected absorbents. The *ung. hydrarg.* is used in the quantity of half a drachm to a drachm night and morning, to be well rubbed in, before a fire, on the more delicate portions of the skin. Cullerier prefers using mercury by friction in primary sores; he orders from a quarter of a drachm to a drachm and a half of mecurial ointment at each friction, leaving an interval between them of one, two, or three days, with the view of not irritating either the sore or the constitution, by bringing the latter suddenly under the influence of the remedy. Ricord frequently orders them to the axillæ, and they

are employed in this manner by Cullerier, in certain forms of ulcerations of the mouth and fauces. He narrates two cases cured by mercurial frictions in this situation, which had resisted its employment in other parts.

Fumigation.—Fumigation of the whole surface of the body is, at present, rarely used as a method of affecting the system, but the apparatus formerly employed is still to be found in some of our hospitals. It is very speedy in its action. The remedy is, however, employed locally, and with gerat advantage, in some affections of the throat and nasal fossæ, directed to the part by a suitable apparatus, and more generally in some obstinate diseases of the skin. For patients who have not strength to rub in mercury, and whose bowels will not bear the use of internal remedies, it has been esteemed highly advantageous.

Iodine.—M. Cullerier thinks that the effects of the iodide of potassium are less prompt than those of mercury, and that, on this account, more should be given, if the stomach will bear it. He employs grain doses of iodine with from two to four of the iodide of potassium in an ounce of water, given at intervals during the day; but he does not increase the iodine beyond two grains in the day, or the iodide beyond ten. I fully believe that the iodide is much more beneficial without the pure iodine, which disorders the stomach without benefitting the complaint. Mr. Stone, formerly apothecary to St. Thomas's Hospital, told Dr. Williams that he was called to prescribe for ten patients taking the compound of iodine and iodide of potassium for one that was taking the last medicine only.

Dr. Wallace found by experience that the iodide of potassium was the only form of the remedy which agreed, that pure iodine was a very powerful irritant, very frequently occasioning severe symptoms, whilst the iodide of potassium was quite harmless. Pure iodine, moreover, is converted into hydriodic and in the stomach. He has seen many cases in which the tincture of iodine, both simple and iodurettted, failed to produce any favorable influence, because the irritation excited in the stomach prevented its employment in such doses as were sufficient to act on the disease, and in these very cases the action of the iodide of potassium was subsequently most beneficial. In other cases, where pure iodine was employed, although the disease was cured, still it was at the expense of an injured stomach, and great emaciation. On the contrary, he asserts that he has never seen unpleaant

effects result from the iodide of potassium, except from mismanagement.

Ricord employs the iodide of potassium in gradually increasing doses, commencing with ten grains dissolved in three ounces of distilled water, and given at intervals during the day, in any suitable vehicle. According to effects so must the dose be either increased or diminished;—when the remedy agrees, which it always does, if the stomach be healthy, the dose should be increased ten grains every two or three days, till it is carried to one and one and a half drachms, or even more, in the coutse of the day. The iodide of potassium, in full doses, when it agrees, occasions a sensation of warmth in the stomach, improves the appetite, accelerates digestion, so that more grow quite fat, and quickens the pulse. A constant effect is an increased diuresis.

When pure iodine is used, or the iodide given in excessive quantities, or from idiosyncracy of constitution, unpleasant symptons may arise. Sometimes those are slight, and resemble a common catarrh; at others, ringing in the ears, and pain in the head, or the skin may suffer from a slight pustular eruption; occasionally it disorders the bowels, or produces pain or uneasiness in the stomach, having some resemblance to pleurodynia, but seated more deeply, and an acrid dryness of the throat. Mr. Mayo says that we may sometimes correct these symptoms by adding a few drops of laudanum to each dose, and by administering aperient medicine. Authors assert that some patients experience idiotic intoxication, characterized by a slight uncertainty in the voluntary movements, some *subsultus tendinum*, heaviness in the head, a species of intellectual idleness. and sometimes delirium. Soreness of the gums and ptyalism are also said to occur occasionally. Mr. Mayo has heard of effects resembling mercurial erythismus. Should any of these symptoms occur in a severe degree, the dose must be diminished, or even abandoned altogether for a few days, and its exhibition re-commenced in smaller doses.

Dr. Wallace found the urine to be the best test of the effects of the iodide of potassium on the system, by testing it with starch, &c. In some of his patients he remarked a great increase of perspiration—sometimes constipation, salivation, roughness of the throat, and heartburn; he found that quinine controlled the state of the throat and stomach. Delicate females, he says, sometimes lose the power of sleeping so much as is natural—a state of wakefulness often accompanied by peculiar feelings of the head, which is relieved by a purgative

and interruption of the medicine. Emaciation, great gastric irritation, wasting of the mammæ and testes, &c., only occur from the free use of iodine. In two patients who had drachm doses of the iodide of potassium administered by mistake for one day, there occurred in both sickness, soreness of the throat, colicky pains, vomiting and purging to a slight degree, frequent pulse, and exhaustion, quickly disappearing. Several patients, while under the full action of the iodide, were attacked with an acute pain in the anterior and lower part of the left side, precisely in the centre of the superficeis formed by the false ribs, accompanied by some cough, difficulty of breathing and fever. In all, the affection went off without much trouble. The medicine was omitted and subsequently resumed without inconvenience. In a private patient it produced severe indigestion, a rapid and quivering pulse, headache, and a peculiar condition of the eyes—the pupils were dilated, and both eyes in a state of incessant motion. He was soon after seized with symptoms of paralysis on one side of his body, preceded by muscular tremblings, which remained for three weeks, but eventually passed off.

Ricord states that the good effects of the iodide of potassium have been constant in his practice, but not produced with equal rapidity, in this respect differing from Mr. Mayo, who says that no medicine, where it does good, produces amendment so rapidly; therefore the propriety of continuing it is never doubtful. As far as I have observed myself, iodide of potassium never gives rise to any serious symptom, provided that it be unmixed with pure iodine, and be administered in moderate doses. Apoplectic and paralytic symptoms sometimes come on during the existence of tertiary symptoms, and these are then attributed to the mercury or the iodine which the patient may be using at the time, but it is hardly fair that the whole blame should fall on the remedy. For an adult it is sufficient to commence with five grains of the iodide of potassium three times a day, and increase it gradually to seven or eight. Dr. Williams, while he admits that some constitutions are affected even by one or two grains, thinks that the average dose should be eight grains three times a day; for, says he, a smaller dose can hardly be recommended, for the patient's sufferings are so intense as to require immediate relief, and consequently we ought to begin with as large a dose as his stomach will probably bear. This reasoning is not altogether conclusive, for if the dose be sufficient to excite or endanger unpleasant symptoms, we shall have to stop its use altogether for some time, and then to resort to smaller doses,

which, if used at first, would most probably have removed the complaint without any distress or delay. Dr. Williams remarks, that when mercury has been previously and successfully used, the quantity of the iodide necessary for the cure of the patient is often much greater than where none has been exhibited.

Review of the comparative value of Mercury and Iodine in the treatment of Syphilis.—If we take a review of what has been written, we can readily determine the comparative value of mercury and iodine in the treatment of syphilis—that mercury and iodine form the two main remedies on which the best and most unprejudiced treatment of the various symptoms and stages of syphilis mainly hinges, although neither of them should be regarded as a specific, nor can either of them, to be used well and successfully, be exhibited empirically;—that mercury and iodine, when guided by observation, reason, and experience, and combined with such treatment and medicines as the profession would employ were they to lay aside all notions of something specific requiring a blind and specific use of some remedial agent, they stand alone, and infinitely superior to all other medicines which the materia medica can furnish; that a modified use of mercury is adapted to nearly all the forms, but especially the indurated, of primary syphilis; that in constitutional syphilis a modified use of mercury is almost a *sine qua non* in the great majority of secondary symptoms, but is either hurtful or useless in the tertiary; that iodine is inert in almost all the symptoms of primary syphilis, with the exception of some forms of phagedena attended with great debility and derangement of the health; that in constitutional syphilis it is a less valuable remedy to the majority of secondary symptoms than mercury, with the exception of some severe cases of pustular eruption, phagedenic throat, rupia, and secondary ulcerations of bad character, all of them marked by a cachhetic and debilitated constitution; while in tertiary symptoms iodine is far more valuable than mercury, and its effects more certain and decided than in any other set of symptoms;—that mercury and iodine are most advantageously combined in cases presenting both secondary and tertiary symptoms;—that many forms of mercury, having local or constitutional actions, are applicable to the various symptoms of syphilis, but that the mildest constitutional effect, capable of overcoming the disease, is always to be preferred;—that the only form of iodine safely applicable to the treatment of syphilis, is the iodide of potassium, which should never be carried beyond moderate doses; hence, however valuable the

iodide of potassium may be in some forms of syphilis, it cannot be substituted with advantage for mercury in the great majority.—Braithwaite from *Edin. Med. Surg. Journal.*

Pain of the Loins.—By Dr. Oke, Southampton.—Perhaps there is no symptom more commonly met with in practice than pain in the loins, which is usually and at once attributed to bile, gravel, or rheumatism; but as it may also be derived from other causes left out in a hasty decision, I shall enumerate them, and endeavour to point out the symptoms by which each may be distinguished. Pain of the loins may be derived from the muscles, from the liver, from the duodenum, from the kidneys, from the colon, from the uterus, from the aorta, from the spine, or from matter collected on the psoas muscle independent of spinal disease. In order to arrive at its true cause, we must endeavor to ascertain what function is principally involved, which will at once lead us to it.

If the pain be rheumatic, it will be increased by pressure, and by the action of the muscles affected. There will also be rheumatism in other parts of the body, the system will not evince much disorder, the urine will be high colored, and deposite a lateritious sediment.

If derived from the hepatic function, the pain will shoot upwards along the splanchnic nerves to the scapulæ; the alvine evacuations will be either deficient in, or exuberant with, bile; or show a morbid quality of that secretion; the urine will have a bilious tinge; there may be congestion of the hæmorrhoidal veins; and the spirits will de depressed.

If from the duodenal function, three or four hours after a meal the pain will be aggravated, shooting through towards the right side of the abdomen, and remaining till the food has passed into the jejunum. Dyspeptic symptoms will prevail, and there will frequently be painful pustules breaking out about the face. I have lately met with a case in which the boils were extremely annoying.

If from the kidneys, the pain will shoot down the course of the spermatic nerves to the round ligament in the female, and towards the testis in the male, which will often be retracted by the action of the spermatic nerves upon the cremaster muscle. There will be more or less irritation communicated to the mucous membrane of the bladder. The urine also will be diagnostic in this instance· it may deposit mucus, calcu-

lous matter, blood, pus, or albumen, according to the nature of the case; or it may be otherwise morbid in its constitution.

If from the uterus, the pain of the back will arise either from disordered function or disease of that organ. In the former case the pain will be of a neuralgic character, will return in forcing paroxysms extending around the hips and hypogastric region, will be attended with hysteria, and often with increased quantity of the menstrual discharge. In the latter case the pain will be *constant* and severe, extending along the anterior crural nerve half way down the thighs. There will be a thin offensive discharge from the vagina. The continuance will be wan and sallow, exhibiting the wear and tear of organic lesion.

If from the colon, there will be constipation, and inflation in the course of the bowel, or the fœcal discharges will be of small diameter, or there will be soreness of the intestine under pressure, especially at its ascending or descending portions, accompanied by mucus, or shreds of lymph in the form of boiled vermicelli, amongst the excretions.

If from arterial dilatation, an abnormal pulsation of the vessel involved—the aorta, for instance—may possibly be detected by auscultation in the incipient stage of the disease, *if such were suspected;* but in a large majority of cases such a cause may reasonably escape the attention of the ablest surgeon, from there being no tangible symptom that might lead him to suspect it; and even after the dilatation has considerably advanced, it may be sufficiently large to press upon and disturb the spermatic nerves, but not large enough to project and pulsate externally, and this may, at this stage, be confounded with diseases of the renal function. A few years ago I met with a case of this kind in a man of middle age. The pain had been constant and wearing, shooting from the loins down the course of the spermatic nerves, and for a considerable time was reasonably attributed to the renal function, especially as there had been constant disturbance of this function. At length the aneurismal sac began to approach the surface, and then, of course, the cause became apparent.

If from disease of the spinal column, the pain will be aggravated by percussing the spinous processes at this part of the spine, or by suddenly striking the toes against an uneven surface. There will be involuntary action of the muscles, especially of the flexors of the legs, diminished temperature, abnormal feelings, and more or less loss of power of the lower limbs. Should there be at the same time any unnatural

projection of the spinous processes, the disease will be confirmed.

If from a collection of matter upon the psoas muscle, unconnected with spinal disease, the pain will be continued, dull, and deep-seated, extending from the loins down the psoæ, or in whatever direction the matter may have taken its course. The pain will be aggravated by flexing the thigh towards the abdomen, and there will be difficulty in walking; moreover, there will be marks of a strumous habit, and more or less symptoms of hectic fever. Should any fluctuating tumour present at the groin, or at any other point where the matter may find its way out of the body, it will be conclusive as to the nature of the case.—Braithwaite from *Prov. Med. Jour.*

Effects of Camphor.—M. Raspail in his valuable lectures on the physiology of health and disease, says on this subject—During the last five years, I have been in the habit of smoking and inhaling camphor, under the form of a cigar, both day and night; I have also placed every night, under my bolster, a certain quantity of purified camphor. My nights, instead of being agitated, have been passed in a calm and uninterrupted sleep. Indifferent dreams, recalling but the ordinary scenes of life, have succeeded to terrific nightmares, which used to torment me, almost every night, for at least a quarter of an hour. Whenever I awake, I chew from fifteen to twenty *centgrammes* (3 to 4 grains) at least of camphor, which I afterwards swallow, along with a small quantity of water; this sometimes amounts, in the course of the night, to as much as sixty *centigrammes* (12 grains) of camphor, which I have accustomed myself to swallowing; in the day-time, I often take a dose of similar strength; as an hygienic precaution, I also use frictions of camphorated spirits, when rising or going to bed, and whenever I perceive the least lassitude of spirit, or the slightest exhaustion of body. And with this inflammatory treatment according to the Brownian, the Rasorian, and physiological doctrines, I never was better in my life, nor, in fact, so well for a long time together; I have entered on a new kind of existence; I have, so to speak, shed the old skin of disease; I have grown young again in physical and moral strength; I am more disposed to labor, and am less inconvenienced than ever by it. I therefore consider myself justified in recommending others to partake of the benefits derived from this long and conclusive trial. My own family, as well

as numerous patients, can bear out my testimony as to the immense advantages they have derived, from this medicinal agent. I should add that constipation is, generally speaking, produced by medicines of this class; this constitutes the reverse to their good effects, to the activity which they excite in the digestive organs, and the appetite to which they give rise.—Braithwaite from *Med. Times.*

Antidotes of Corrosive Sublimate, Copper, Lead, and Arsenic.—By MM. Bouchardat and Sandras.—By means of numerous experiments, first made in the laboratory and then repeated on dogs, MM. Bouchardat and Sandras have arrived at many interesting results relative to the antidotes of corrosive sublimate, copper, lead, and arsenic. All these are detailed at length in their long papers on the subject. Their conclusions were, that the following substances may be regarded as antidotes, and employed as such in medicine:—

As antidotes for corrosive sublimate—A mixture of zinc and iron filings; or powder of iron reduced by hydrogen; or the moist persulphuret of the hydrated peroxide of iron.

As antidotes for copper.—A mixture of zinc and iron filings; iron reduced by hydrogen; porphyrized iron; zinc filings; or the persulphuret of the hydrated peroxide of iron.

As antidotes for lead.—The moist persulphuret of the hydrated peroxide of iron.

As antidotes for arsenic.—The moist hydrated peroxide of iron; the dry hydrated peroxide of iron; and the moist persulphuret of the hydrated peroxide of iron.

These experienced chemists add the following reflections:—This last preparation, the persulphuret of the hydrated peroxide of iron, possesses this superior advantage over all the rest, that it changes the nature of all the four poisons above noticed, and is especially applicable in those cases where we have not had time to find which of the poisons has been taken. As to the manner of administering these antidotes, and the doses which it is necessary to administer, the simplest means appear the best. The powders of zinc and iron may be suspended in any electuary, or they may be swallowed in wafer paper. The magma of the hydrated preparations of iron may be swallowed in the form of jelly, in which they are procured from the druggists. Several draughts of lukewarm water ought to follow the antidote, and the fauces be tickled

with a feather, to excite vomiting and the expulsion of the poison. The efforts at vomiting scatter more effectually over the stomach the antidote which is administered. As to dose, the experiments proved that 100 grains of the powder of iron or of zinc sufficed to prevent any bad effects from 15 grains of the acetate of copper. Fifteen drachms of the moist magma of the persulphuret were required to produce the same effect with the same dose (15 grains) of the acetate of copper. To act as an antidote to 4½ grains of arsenious acid, 15 drachms of the moist magma of the persulphuret, or 30 drachms of the moist hydrated peroxide of iron, or 20 drachms of the dry hydrated peroxide of iron, were required. With regard to the time when these antidotes can be administered with advantage, in so far as the acetate of copper is concerned, the lapse of 40 minutes from the time of swallowing the poison ought not to be regarded as a sufficient reason for not administering the antidote; but arsenic is more quickly absorbed. Nevertheless, the antidote should always be administered, because, though it will not neutralize what is absorbed, it will prevent its further absorption, by decomposing what remains in the stomach.

Braithwaite from *Edin'g. Med. Times.*

Brodie on the best mode of administering mercury in Syphilis.—But if you inquire which is the best way of giving mercury in cases of syphilis where the symptoms are not of the very mildest character, I must say that mercurial inunction is infinitely to be preferred to mercury taken by the mouth. Mercurial inunction, however, is dirty, laborious, and troublesome, and it makes the case public to the family in which the man lives. For these reasons it will be objectionable to the patient; but it has this advantage, it is much less liable to gripe and purge, and it cures the disease a great deal better. It does not damage the constitution half so much as mercury taken by the mouth; nay, I will go so far as to say that, except in the very slightest cases, you really cannot depend upon any mercurial treatment effecting a certain cure, or even giving a good chance for it, by any other means than inunction. You may often patch up the disease by giving mercury internally, but it will return again and again and again, and you may cure it again by a good course of mercurial ointment. But especial care must be taken that this is proper-

ly applied. If it be left to a patient he will rub it in for five minutes or so, whereas it requires to be rubbed in before the fire for three-quarters of an hour ere it enters; but by and by the friction may be continued for a shorter period. Where the symptoms are not of the mildest character it is desirable that the patient should, if possible, be confined to the house. Mr. Pearson observed, long since, that going into the fresh air would undo the effect of mercury, and I never would be responsible for thoroughly eradicating the disease where the patient is at all exposed to cold, and where he does not lead a most careful and regular life.

Wherever you can in the treatment of syphilis, make the patient take mercury in the form of inunction if possible. It is the best plan to pursue in all cases, although it is not necessary in all cases; but where the symptoms are severe, and a long course is required, it is the safest mode of proceeding.

I will avail myself of this opportunity of stating a class of cases in which you may employ mercurial inunctions with the greatest advantage. Children when born, sometimes labor under syphilis, the father or mother having been affected with it—perhaps the father and not the mother. The child at birth looks thin, and is of small size, and instead of thriving it becomes still thinner. At the end of three weeks it is covered by a nasty scaly eruption; there is a sort of aphthæ in the mouth, and chaps about the lips and anus. I have tried different ways of treating such cases. I have given the child grey powder internally, and given mercury to the wet nurse. But mercury exhibited to a child by the mouth generally gripes and purges, seldom doing any good; and given to the wet nurse it does not answer very well, and certainly is a very cruel practice. The mode in which I have treated such cases for some years past has been this—I have spread mercurial ointment, spread over a flannel roller, and bound it round the child once a day. The child kicks about, and the cuticle being thin the mercury is absorbed. It does not either gripe or purge, nor does it make the gums sore, but cures the disease. I have adopted this practice in a great many cases with the most signal success. Very few children recover in whom mercury is given internally, but I have not seen a case where this method of treatment has failed.—*Braithwaite's Retrospect.*

On the utility of Surgical Operations in Cancerous Diseases. The grand points of this most important surgical question are to determine—1, if it be really true that cancerous disease is primarily of a local nature, and subsequently degenerates into a constitutional malady; and 2, if extirpation, performed at an early period, prevents the occurrence of this degeneration. With the view of elucidating these matters, M. Leroy d'Etiolles has collected the following statistical observations. Of 801 operations, 117 were performed within a twelve month after the first manifestation of the disease.

Of these 117 cases, there were 61 in which the disease had returned at the time when the reports reached me. It is more than probable that this proportion would be found to be still higher, if we knew the actually present state of these cases.

The results of operations for Cancer of the Lip are curious and worthy of notice, in consequence of the differenee in this respect observed in the two sexes. Of 633 cases of Cancer in the male subject, 165 were examples of Cancer of the Lip; of those 114 were treated with the knife—12 with caustics. There were 15 relapses in all; that is, about an eighth of the whole. On the other hand, of 2,148 cases of Cancer in the female, there were only 34 instances of the disease in the lip; of these, 22 were treated by excision; and in seven—nearly a third—there was a return of the disease.

This difference does not hold good of Cancer of the tongue; for then the disease is equally fatal in both sexes. Of nine operations, in which a cancerous tumour of this organ was extirpated, three were performed within one twelve-month after the earliest appearance of the disease. In the other six cases, the patients died, the disease having previously returned.

As respects Cancerous diseases of the Mamma, we find the following data. Of 277 operations, 73 were performed within the last two years: as yet we cannot say positively what are the results. Of the remaining 204 cases, 22 of them proved fatal in the year after the operation, and in 87 others there was a relapse of the disease.

M. Leroy deduces the following conclusions from his researches:

1. Extirpation does not arrest the progress of Cancerous disease.

2. This operation should not be resorted to, as a general method of treatment, except for Cancer of the skin and lips.

3. There is no necessity to extirpate Cancerous disease of

other organs, except when an alarming hæmorrhage supervenes.—*Comptes rendus.*

The Academy appointed MM. Roux, Velpeau, and Serres, to report upon this communication of M. Leroy.

Med. Chir. Rev.

Use of Colomba-Root against Vomiting.—This well known and long established drug is an admirable remedy in many cases of ataxic and nervous vomiting. A writer in a recent number of the Annali Universali, mentions the case of a soldier, who was suddenly seized, after a fright, with violent pain in the abdomen, accompanied with vomiting and diarrhœa. The other symptoms gradually ceased; but the vomiting, (which was evidently of a nervous character,) still continued. The patient had a yellow cachectic look, a furred white tongue, and loss of appetite; slight epigastric pain usually preceded the coming on of the fits of vomiting. They generally returned six or seven times every day. Every thing tried failed in checking this obstinate irritability of the stomach, until seven months after the commencement of the disease—Dr. Monfredonia suggested the use of Colomba; it was given in the form of powder, and in the dose of eight grains four times in the 24 hours, in a small cupful of goats' milk. A very sensible amendment in the state of the patient speedily took place, but the vomiting did not entirely cease for some time. The Colomba was subsequently given in the form of the decoction; and under its use the health was gradually, but completely restored.

Colomba is an excellent remedy in a great many cases of gastric ailment: a favorite formula of ours is—Infusi Colombæ ℥ vj., Liq. Potassæ ʒ iss., Tinct. Humuli ʒ iij., Aquæ Pimentæ ℥ iss. M. It is often very useful in phthisical patients.—*Ibid.*

Quackery.—A fatal case of empirical treatment lately took place in Wurtsboro', (N. Y. we suppose), an infusion of tobacco and the entrails of some animal having been, it is said the active agents in the hands of the quack whose name was John Hollister.—*Ibid.*

Quinine against obstinate Hiccup.—Among the many anomalous, and not unfrequently very obscure, results of miasmatic influence, are various affections of the digestive organs. Of these, Hiccup is a most troublesome, and, occasionally, a very intractable one. In some cases, this gastric disorder has been observed to exhibit a distinctly intermittent character, and to be associated with the existence of other phenomena which are usually attributed to the operation of the same morbific cause. M. Mendiere has recently published, in the pages of the Revue Medicale, several cases of this sort, in which the distressing symptom of Hiccup was promptly and decisively cured by the free use of quinine, after it had resisted every other mode of treatment. He has used the same remedy with good results in many cases of severe Cardialgia.

(The addition of a few drops of tinct. opii to the quinine will greatly enhance its efficiency in such cases. What an admirable remedy opium is in almost all the Neuroses, provided the secretions are healty at the time! The difficulty lies in knowing how to time its administration, and regulate its dose. We have usually found the liquor opii sedativus the form in which to exhibit it: this may be given in a bitter infusion, to which ammonia may generally be added with advantage at the same time.)—*Ibid.*

The Human Brain.—The weight of this organ in adult white males, varies from 3 lbs. 2 oz. to 4 lbs. 6 oz. Troy weight. The brain of men distinguished for their intellectual power is usually large. That of Cuvier was one-third larger than an ordinary brain. It weighed 4 lbs. 11 oz. 4 dr. and 30 grains. The brain of idiots is usually small. That of an idiot 50 years old, weighed but 1 lb. 8 oz. and 4 dr., and that of anather aged 40, 1 lb. 11 oz. 4 dr.

The male brain is on the average heavier than the female, yet in proportion to the weight of the whole body, it is rather less heavy. The intellect is not absolutely proportioned to the size of the brain, but it is proportioned to the size of the hemispheres, and especially to the extent of their surfaces. In emaciation this organ does not diminish in proportion as the rest of the body does. Hence in many diseases accompanied with great emaciation of body the mind remains active and vigorous.—*Jour. of Insanity.*

Psychological Medicine.—Prof. Otto has lately written on the "Action of different Medicines on the Mental Faculties." He says that ammonia, opium, musk, castor, wine, ether, and the preparations of gold, enliven the imaginative powers, and render the mind more fertile and creative, while the empyreumatic oils, iodine, arsenic, belladonna, and conium, are apt to induce a tendency to melancholy, and to diminish the energy of the intellectual powers.—*Ibid.*

On the Protective Influence of Vaccination.—The general conclusions, drawn by Dr. Retzius of Stockholm from his observations of small pox and the effects of vaccination in Sweeden, are these: "The protection, afforded by vaccination from the close of the second year of life against the contagion of the Variolous poison, usually lasts unimpaired to the end of the thirteenth year or so: after this period it begins to lose its effect, and gradually becomes more and more uncertain on the twentieth or twenty-first year of life. For the next four or five years, the disposition to the small-pox seems almost to have recovered its original integrity; and this state of liability continues unimpaired up to the age of forty years or so. At about this epoch of life, it begins to approach nearer to the limit of its existence—which it reaches, in the majority of cases, about the fiftieth year,—the period when the general revolution of the human body commences to take place."

(The practical inference to be drawn from these remarks is the propriety of repeating vaccination in about thirteen or fourteen years after its first performance. This advice is in accordance with the observations of the most experienced practitioners: it would be well, if it were more generally acted upon.—*Med. Chir. Review.*

Combe on the Influence of Sleep in warding off Insanity.—"If those who are exposed to any of the exciting causes of cerebral disease, or of insanity, put themselves on their guard to secure regular sound sleep, they will do much to ward off an attack. The moment the cause begins to excite sleeplessness by night, and restlessness by day, with an involuntary propension of the mind in one direction, at first perceptible perhaps only to the patient himself, it is time to take alarm, and if possible, remove or counteract its agency. If it is ex-

cessive application to business, continued anxiety of mind, or excess of study, that is keeping up the activity of the brain, and placing it on the verge of disease, this may often be prevented by timely relaxation, or removal from the scene of anxiety, and particularly by carrying off much of the nervous energy in abundant muscular exercise often repeated, and by rigidly abstaining from mental exertion at night, and thereby allowing the brain to fall into that state of quiescence most favorable for repose. I have seen some striking instances of the efficacy of this plan in restoring tranquillity of mind, when on the very verge of derangement. The excitement of company and of tea, sometime resorted to in such instances, may, if carried to any length, only add fresh fuel to the flame, and stimulate the brain beyond recovery; but the society of those whose feelings and pursuits are calculated to soothe those most excited in the patient, and to call others into action, is very beneficial."—*Ibid.*

Useful Hints to Hospitals.—In the hospital at Frankfort, I noticed a peculiarity in the make of the mattresses which deserves to be noticed. They consist of three portions or compartments, which are perfectly joined into one when the bed is made; but which are readily separated from each other, so that any part may be removed and cleaned at a time without the others. As a matter of course, it is the middle one that generally requires to be so treated. By this simple arrangement, the mattresses may be, at a very small expense, always kept in cleanliness and order.—*Med. Chir. Review.*

THE WESTERN JOURNAL.

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NEW SERIES.—VOL. II.—NO. V.

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LOUISVILLE, NOVEMBER 1, 1844.

TRAVELING LETTERS FROM THE SENIOR EDITOR.

NO. VII.

PEORIA, Illinois, September 17, 1844.

To my Colleagues:

GENTLEMEN:—On my way to this place, through several towns of this State, I heard much of the

Epidemic Erysipelas,

but saw no case, at least none that was well characterized. It prevailed last winter and spring, but like scarlatina, passed by many localities, while it fell heavily on others. Believing that our readers would bear another communication from me on the experience of the profession in this quarter, I was about to make one, when a newspaper containing an account of the disease, as it appeared and was treated in and around Bloomington, M'Lean county, east of this place, fell into my hands, and will be found better for our readers, than any thing I could serve up for them. It embodies the experience of our friend Dr. John F. Henry, a sound practitioner and most candid narrator, and therefore merits a more permanent place than that of a

village newspaper. I send you the paper and request its insertion at the close of this letter.

Since I left the Illinois river at Mendocia, a fortnight since, and proceeded by way of Jacksonville, Springfield, and Bloomington to this place, I have every where met with

Autumnal Fever,

which prevails as much as it has ever done before, but in general is a mild tertian or double tertian. Now and then a malignant case presents itself, the collapse in which is as fatal as was that of epidemic cholera, and quite as easily averted by timely and appropriate medication. The disease is by no means confined to the watercourses, but occurs in the depths of the high rolling praries.

The meridian in which I now am, does not more abound in autumnal fever, than in

Medical Schools.

In St. Louis there are two, in Jacksonville one, and in Chicago another, making four, all commenced within as many years, in a region which a short time since was the Far West.

The oldest is the *Medical Department of Kemper College*, St. Louis, projected by Professor McDowell, and known in the surrounding country as 'McDowell's School,' that gentleman being at once its Achilles and Ulysses. It has a respectable edifice, and its last class amounted, I understand, to 60, chiefly practitioners and students from Missouri and Illinois.

The next in age, is the *Medical Department of the University of St. Louis*, founded by Professors Hall and Prout, seceders from the last; but Professor Prout's withdrawal from this, also, leaves Professor Hall, in the undivided honor of being its projector. Less distinguished for public speaking than his rival, the Professor seems to possess a quiet performance, the fruits of which are the organization of a full Faculty, and the erection of a suitable edifice, which will be ready for occupation by the ensuing November. Its class last year was, if I recollect right, between 20 and 30.

I am acquainted with, and might say something, of a part of the Professors of each of these young colleges, but as 'I have not come,' either 'to praise Cæsar,' or 'to bury him,' I shall content myself with adding, that the care of the Hospital, is confided to the Faculty of one school for half the session, and to the other for the remainder;

the pupils of each having equal access to it throughout the winter.

The next of which I shall speak, is the *Medical Department of Illinois College.* As I passed through Jacksonville, I made acquaintance with the only two of its Professors who reside in that town, Dr. Jones and Dr. Adams. The latter, who holds the chair of Chemistry, Materia Medica, and Therapeutics, is also Professor of Chemistry and Natural History, and teacher of Modern Languages in the Academical Department; Dr. Jones, Professor of Obstetrics, appears to have been the founder of the establishment. The other Professors, reside, I believe, in different places. Jacksonville has no hospital, but the faculty promises the advantages of a dispensary to their pupils. The class of last winter, according to the printed catalogue, numbered fourteen. Cost of tickets $60.

As I have not yet reached Chicago, I can only speak of its school from an advertisement, which, a few days since, I cut out of a newspaper. Its title is the *Rush Medical College,* its Professors five in number, and the price of all their tickets $60. Dr. Brainard stands at the head of the faculty in this advertisement, and may be taken, I suppose, as the projector of the enterprize.

Multiplication of Schools.

Every where that I go, our brethren are crying out against the multiplication of medical schools. But to what end? Who can arrest it? The medical school diathesis of the profession and the States, is like some morbid actions, 'self-limited,' and must be left to correct itself. It is of no use to be croaking about 'their increase,' but we may and should complain of under-bidding; and to see, as in the schools of Illinois, the number of Professors reduced to five, and the aggregate amount of their tickets to $60, is a cheapening that ought to be condemned. I fancy that the Professors would take exceptions to their brethren's reducing the price of their visits to twenty-five cents, or that of an amputation to 'fifteen shillings,' and with such feelings and opinions of professional propriety, they ought not to sell their lectures at a discount of 33⅓ per cent. If the young men of Illinois cannot afford the ordinary and established expenditure for medical education, they had better devote themselves to the cultivation of its millions of unenclosed and ex-

uberantly fertile acres, and thus create the means of educating the next generation.

By the way, the *country* seems to me, in this State, to be the best locality for a physician. Illinois has no circulating, but a lively *locomotive* medium, which a physician might accumulate around him in abundance. As I was lately riding out with one of the oldest physicians in the State, he met a patient, who insisted on paying his bill in horses or fat cattle, and another took me to his farm, to show me, among other objects, the colts and calves which he had derived from the same source. In this way a physician might soon overspread a grazing farm with stock, and by its means replenish his stock of quinine when exhausted, which cannot always be done, with the whole amount of money received from those who consume it.

Ottowa, September 19, 1844.

Spending half a day in Peru, I reached this place last evening. When at Bloomington and Peoria, I thought I saw evidence that

Autumnal Fever

was less prevalent than in the towns below, either in Missouri or Illinois. At Peru, its prevalence was still less, and here I find, as there, that the people are quite healthy. Thus I have traveled out of the region in which, *this year*, it is specially prevalent. Its northern limit lies a little above the 40th° of latitude. I find, however, that the

June Flood

prevailed here, and that the Illinois river, at the mouth of Fox river, where I am writing, rose higher than it was known by the 'oldest inhabitants,' (a memory of 12 or 15 years!) to have risen before, in the summer. That this rise has not been followed by as much sickness hereabouts as below, may, *perhaps*, be ascribed to a greater elevation, and less width of the 'river bottoms.'

Will you excuse me, if I again embellish a page with

The Paroquet?

The last flock which I saw was a little above the town of Hennepin, about the 41st° of latitude; and Dr. Lombard of this place tells me they do not come north of that limit. As far back as

1781, Mr. Jefferson attached importance to the northern migration of this beautiful bird, in estimating the relative heat of the climates east and west of the Alleghany—drawing from the observation an erroneous conclusion. The delightful Valley of the Illinois, seems at all times to have been the favorite haunt of the paroquet. I have seen more since I came into it, than in all other localities which I have visited this summer; and Father Marquette and M. Joliet, the first Europeans who ever traversed it—more than 170 years ago—saw them in great numbers. If you ask, what Ornithology has to do with medicine? I know of no more appropriate answer than this: the Romans, before going to battle, noticed the flight of birds, and Napoleon has compared a physician, contending against disease, to a General in the field—now if the flight of birds was useful in war, it may be so in physic. Certain, I am, that many flightier prescriptions have been made, than what the flight of birds would have suggested.

JOLIET, September 20, 1844.

I am now in latitude 41° 30′, nearly as far north as the southern end of Lake Michigan, and having finished my inquiries of the medical gentlemen of Joliet, I sit down at my chamber window, 9 P. M., serenaded by the rapids of the beautiful *Riviére des Plaines*, to continue, perhaps close, this philosophical epistle. The first matter to which I ask your attention, is,

Juliet, versus Joliet!

You will *Swiftly* exclaim—

> "Strange such a difference should be,
> 'Twixt tweedledum and tweedledee"—

but don't be too fast. The joint discoverer and first navigator of the Mississippi, deserves at least a *nominal* monument, in some one of its thousand vales, and none more appropriate than that of the Illinois, through which he and his partner returned from their voyage of discovery, in 1617. It was to commemorate this daring adventure, through 1500 miles of perilous wilderness (now the home of more than 1500 doctors), that the proprietor of this town gave it the name of Joliet; which the people, with more gallantry than gratitude, have corrupted into Juliet. I call on both of you, as candidates for fame, to do all you can to restore to the intrepid pio.

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neer the memorial which he is about to lose. Should you think this matter out of your line, I am sure you will not despise that which I am now about to present, under the head of

Triumphs of Mesmerism.

A medical gentleman of this place informs me, that he can at any time relieve spinal irritation, by mesmerism; also, that it is one of the best means of preventing abortion which he has ever tried. He can mesmerize a nervous patient after lying down for the night, and she will sleep soundly till he awakes her, which he can do, by an effort of his will, transmitted from his own house. Still further, he can prevent the paroxysm of an intermittent fever, by mesmerizing his patient before the access of the chill, but the next paroxysm will come on. However, he has discovered (taking the hint from a work on homæopathy), that by producing an artificial chill during the intermission a radical cure is effected. This he accomplishes by producing mesmeric sleep, and then applying his finger to the tip of the patient's nose. Finally, the following case has lately occurred in his practice. A lady was subject to such obstinate constipation, that she passed fourteen days without an evacuation, when a drachm of calomel and six drops of castor oil produced only one discharge. Eight days after that, not having had another, he threw her into the mesmeric sleep, when mesmerizing a glass of water, he gave it to her as a solution of Epsom salts, the taste of which she admitted: he then made her hold half an ounce of calomel in her hand for about 30 minutes, when he awoke her. I do not recollect whether he mesmerized the calomel. When he called the next morning, he found that she had been purged to excess, and was profusely salivated, with swollen jaws and a mercurial breath. He then mesmerized her again, and putting a roll of brimstone (not mesmerized) into her hand, and applying another to her cheek, she was perfectly well in three days, and her costiveness has not since returned.

Consumption in Illinois.

For the last fortnight, during which I have conversed with the physicians of seven towns between Jacksonville and Joliet, I have been assured, *nemine contradicente*, that tubercular consumption is one of the rarest diseases of that part of Illinois which lies be-

tween St. Louis and Chicago. Most of them have not seen an indigenous case, and all have known of immigrants in whom the progress of the disease, in its early stage, had been arrested or retarded. As many of our readers may be consulted by their patients, concerning the effect of the climate of this region on those inclined to phthisis, I have thought it proper to anticipate a future publication by this brief notice.

Very respectfully, your
Obedient servant,
D.

BLACK-TONGUE.—ERYSIPELAS.

Bloomington, Ill., May 20, 1844.

Dr. W. H. Tegarden:

Dear Sir:—Your letter of the 16th ult., requesting my views on the nature and treatment of the disease called the '*Black Tongue,*' has remained unanswered, in consequence of my pressing engagements, growing out of the protracted prevalence of our epidemic. Having, on this day, only four cases on hand, three of which are convalescent, I propose entering into the subject with as much brevity as is consistent with a full understanding of its character and mode of treatment. And as *names* are of much importance in the practice of medicine, influencing, as you are well aware, nine-tenths of the profession, I am compelled to tell you that our disease is not the '*Black Tongue*' at all! This may somewhat abate your anxiety to know about it; but when you learn its history you will say with me that it is one of the strangest epidemics that has ever visited any portion of our country. It commenced its ravages in Pekin, on the south-east side of the Illinois river, early in the last autumn, where its mortality is said to have been very great; but it did not present itself in this vicinity until January, and then in sporadic cases of widely different character. As the season advanced the cases became numerous, and what is exceedingly singular, it seemed to fix on particular neighborhoods, where it lingered, until all who were susceptible of its influence, suffered attacks of greater or less malignity. As it declined in its first locality, other portions of the country fell under its sway. Its progress has been re-

markably slow, and the space over which it has spread is very limited. It is understood that none of the towns on the north-west side of the Illinois have suffered at all, and that the extent of its ravages on the south-east side, has not exceeded a radius of 50 miles. Many believed it contagious; but this opinion has become even here, an 'obsolete idea.' The persons most liable to its attacks have been females either in the puerperal state, or advanced in years, and men of feeble or deranged health. Children and robust persons have escaped almost entirely.

The symptoms were very diverse. It almost invariably commenced with a chill succeeded by active febrile excitement, and accompanied with a variety of local affections, some of which were very troublesome, and all of which created in the minds of the people great apprehension and dread. The most common of these local affections, was an inflamed condition of the tonsils, and of the fauces generally, producing in some instances so great a swelling of the parts as to impede respiration, and almost to obstruct deglutition. This, in two instances which I have seen, extended to the tongue, enlarged it to two or three times its natural size, and this state of things has, I presume, given origin to the name, by which you have heard it called. Erysipelas of the face, commencing on the nose, and extending from this, as a centre, until the whole face and ears, and anterior portions of the head and neck are involved, was also frequent. In those cases which fell under my observation, the erysipelas appeared on the hand and fore arm. In several cases, after having appeared on the face, or in the throat, it fell on the arm pit, producing very painful tumors of the axillary glands, which, after a protracted period, terminated in suppuration. I never saw it on the body, in the first instance, except in a very mild case. In two instances it caused a single blister on the fore finger, about the size of a quarter of a dollar, so nearly resembling a burn, that my first impression was, that it had been caused by contact with the stove. In many cases, the local disease was a most painful affection of the collar bone, or the ancle bone, or of the fingers and toes without any swelling of these parts. In one case as an original disease, and in two of relapse, in one of which the disease had first assumed the character of erysipelas of the face, and in the other of sore throat, a most painful and obstinate rheumatic affection displayed itself. In one most singular case the tonsils were greatly enlarged

and at the same time a very firm, but not painful tumor, of a very indolent character, extended from the ear to the clavicle, evidently composed of enlarged lymphatic glands. These were the primary local affections, but as secondary symptoms, we had inflammation of the membranes of the brain, lungs, and abdomen, the former producing stupor, the latter constituting pleurisy and peritoneal inflammation; and in several cases, suppuration of the tonsils, and of the glands of the arm-pit, and in one a deep-seated abscess beneath the fascia of the thigh.

A disease so eccentric cannot, perhaps, be designated by any name, which would be unexceptionable. That it was epidemic admits of no dispute, as in conformity to the laws which govern this class of diseases, it alone reigned during its prevalence. We know next to nothing of the causes which give a peculiar character to epidemics, but it is very certain that they are greatly under the influence of the common exciting and predisposing causes of disease. Thus women in a peculiar condition, and men of feeble health, were predisposed to our epidemic, and a period of damp, cold weather never failed to excite it in those susceptible of its influence; whereas a few days of sunny and cheerful weather put an immediate stop to its progress; and all who were sick became convalescent. I was in the habit of calling our disease *Epidemic Erysipelas*, and when the local affection was on the surface, or even in the throat or tongue, I conceived the name strictly appropriate. When some deep-seated part bore the burden of disease, the term was equally proper, though its fitness was not quite so apparent. The history of a single case will illustrate this view of the subject.

I was called to a patient with sore throat and high fever; under proper treatment the disease was dislodged from this part, and then suddenly, after a few days' respite, attacked the pleura costalis; from which it retreated to the cardiac orifice of the stomach or to the diaphragm. After hanging on this part, producing a most annoying hiccough for five days, it changed its location and appeared in genuine *Erysipelas* on the face. After spreading for two or three days, during which there was an appearance of convalescence, it disappeared and seized on the membranes of the brain, producing, as I believe is always the case when erysipelas attacks those parts, great stupor, and under the combined influence of such frequent assaults

the poor fellow sank. As in all the instances of epidemic erysipelas of which we have any record, women, in the puerperal state, were most exposed to its ravages; so it has been with us: the disease in such cases uniformly appearing in the form of *puerperal fever.* For some time scarcely any escaped, unless the medical treatment was commenced before the birth of the child, and continued until the milk secretion was fully formed.

The disease was inflammatory, and demanded, throughout its early stage, an active antiphlogistic treatment. In many cases the lancet could not be dispensed with, and I was often compelled to use it repeatedly. The blood was generally buffed. Local depletion was also of inestimable value. I was in the habit of scarifying the tonsils freely, and in the two cases of swollen tongue, I plunged my lancet into it boldly. The relief afforded by the abstraction of even a small quantity of blood in this way was very remarkable. The next remedy in point of time, if not of value, was an active emetic of tartar and ipecac., and this I have repeated several times in the progress of the disease. The next in value was the free exhibition of mercurial cathartics; these were given to change the secretions of the liver, which were always, more or less, deranged. Cooke's pills or a dose of calomel, or calomel combined with Dover's powder, or opium alone, succeeded by a dose of oil or senna, after an interval of twelve hours, were the most common forms of exhibition. These were repeated and varied according to rules now well understood, until the secretions of the liver were changed. Simultaneously I directed stimulating liniments, sinapisms, or blisters to the neighborhood of the local affection, or when it appeared on the surface, the blisters were applied to the part itself. I used the mercurial ointment and lunar caustic as a local application in a few cases, but with no satisfactory results. The lancet, the emetics, the mercurial cathartics, the occasional combination of calomel and opium, and the counter-irritants were the great remedies, under a judicious use of which all cases would, I think, yield if commenced in time. The pulse ranged from 90 to 120, and in some puerperal cases to 160, and I believe in this, as in all other cases, the utility of bleeding was determined by the impression made on the pulse; when it did good it reduced its *frequency* as well as its *force.* I scarcely ever bled without making my patient sit up, an invaluable rule of practice, for which the profession is indebted to Dr. Marshall

Hall, and then I bled to incipient syncope. The tolerance of the loss of blood was in some cases great, in others but little, and I was governed in my use of the lancet by this circumstance. I should perhaps say more about puerperal cases, but there was nothing peculiar in the symptoms or treatment; the disease was, in the two cases which I saw, formed before the birth; most generally, however, it came on within three days after it, and then with a sudden chill, followed with rapid pulse, tenderness of the abdomen, headache and indomitable thirst. The lancet should be used in the first paroxysm, if at all—and hence the necessity of apprizing the friends of the danger that they may send for their medical attendant instantly on the occurrence of the chill. Calomel alone, or combined with anodyne sudorifics, could not be dispensed with, and the remark made by Dr. Gooch was confirmed by my observation, that where ptyalism took place recovery was certain. But whether this was the cause or the indication merely of amendment, I confess my inability to decide. I ought perhaps to add, that the tendency in this epidemic to abortion was very strong, and in all the cases of which I have heard any particulars, the danger from hæmorrhage was alarming. The tongue was generally heavily coated with a moist white or yellow fur, becoming in bad cases dark, dry, and cracked. All of the symptoms pointed out the great derangement of the chylopoiœtic viscera, and any practice which overlooks the morbid condition of these organs must be exceedingly defective. In a few words, the circulation should be reduced, when it is too active, by the lancet; the stomach should be emptied by emetics, and the bowels freely and repeatedly discharged by mercurial cathartics; the local affection, whatever it is, should be attended to, by its appropriate remedies, already referred to, but it so invariably followed the condition of the system, that an improvement of this never failed to remove them.

I should not omit to state, that I saw a number of cases in which, during the progress of the disease, after the subsidence or removal of the more active stages, the circulation became so languid that I was compelled to resort to stimulants and tonics in free doses. But this took place most generally in cases which had been suffered to run their course pretty much without control. I would add further, that when the face was the seat of erysipelas the danger was generally much less, than when it siezed on the throat, and in either sit-

uation the danger was comparatively less than when it fixed on a vital part. The danger in all cases was clearly indicated by the pulse; when this remained but slightly affected the disease was generally mild. I may add, that I saw no tendency in wounds or accidents to produce erysipelatous inflammation. While the disease was upon our borders, and during its prevalence, I saw a case of compound fracture of the leg, and one of gun-shot wound, besides having performed three operations of an important character, to say nothing of having used the lancet many times a day, and in none did I witness the slightest tendency to the disease. The only exception was an old lady who had been badly burned, and who had heretofore suffered with erysipelas.

In conclusion, I ought to state, that when the disease assumed the rheumatic form, I found no change of remedies required, except a greater reliance on blisters, and a more free use of sudorific anodynes, combined with mercurials.

Should you deem this communication of any use to the people of Kentucky, to whom I am under so many obligations for past kindness, I cannot object to its publication, although it has been hastily and somewhat carelessly written.

I am, respectfully, yours, &c.

JOHN F. HENRY.

TRAVELING LETTERS FROM THE SENIOR EDITOR.

NO. VIII.

CHICAGO, Ill., Sept. 24th, 1844.

To my Colleagues:

GENTLEMEN:—My last, finished at Joliet, only four days ago, has not reached you, before I begin another. But don't be alarmed, for I shall not close this under a week. The present and prospective importance of Chicago, resulting from its geopraphical position, seems to give it a claim to the attention of travelers of all sorts, beyond that of many other towns; and I am unwilling to leave it, without preparing a brief notice of such 'matters and things' concerning it, as may, by 'liberal construction,' be introduced into a

medical journal, as part of a rambling letter from a rambling editor.

On approaching the town from the west, the traveler descends from the high, undulating prairies, over which he has been rapidly rolled along, towards a small river, called by the French pioneers, *Des Plaines*, which coming down from the north, parallel to the lake, bends to the south-west to unite, some miles below the crossings, with the Kaukakee, which flows from the south-east. The junction of these streams, forms the Illinois, as the Allegheny and Monongahela unite to form the Ohio. For a few miles before arriving at the Des Plaines, one finds himself traversing a level prairie, elevated a few feet above the surface of the little river, and after fording it, he continues his journey over the same kind of surface for ten or twelve miles, when he reaches the new city, whose streets rise but six or eight feet above the surface of the Lake. This extensive plain, which has suggested the expressive name of the river which flows through it, is not without some declivity both towards the river and the lake; but, nevertheless is so flat, that the rank vegetation which it supports, prevents the rain from flowing off, and thus creates a permanently wet, if not marshy surface, which, however, a little ditching would render sufficiently dry for cultivation. As yet this has been done but to a very limited extent, and one is surprised to see, for ten miles before him, the spires, and masts of a beautiful and thriving commercial town, while he is still in the midst of the 'pontine marshes.' But he need not dread an algid or soporose intermittent, for even the simple, is almost unknown both in Chicago and its neighborhood.

The harbor of the town consists of the estuary of a small river called the Chicago, which consists of a north and south fork, meeting within the limits of the town. Between the latter, and the Des Plaines river, which are nearly on the same level, there is such communication, in times of great rains, that canoes have passed from one to the other. Indeed, when the lake was only twenty feet higher than at present, it covered the plain which I have described, and a great river flowed from it down the Valley of the Illinois, to disembogue in the Gulf of Mexico. In the States of Pennsylvania, Ohio, Indiana, and Illinois, there is no other spot where the watershed between the lakes and the rivers of the South, is so depressed as

at this point; hence it was the last to transmit the waters of the lake to the Mississippi, and of the whole, it most favors a navigable communication between the North and South. The canal, partly excavated by the State of Illinois, passes through this depression, and when finished will be the best water highway between them that can ever be made, except, perhaps, that between the Wisconsin and Fox river, which discharges its waters into the head of Green Bay—a route that will forever possess the distinction of having been traversed by Marquette and Joliet, in the voyage which led to the discovery of the Mississippi.

Chicago is the head of navigation on Lake Michigan; the port of embarkation for travelers from the south-west, who would reach New York or Boston by Mackinac and Detroit; the place of debarkation for travelers going in the opposite direction, and for emigrants from the northern States to Illinois, Iowa, and Missouri. You will not be surprised, then, to learn, that in 13 years, it has risen from a few families, to a population of nearly 10,000; that it revels in high anticipations; and cherishes quite as much ambition as any other precocious American city.

Now, lest some of our readers, desirous of changing their location, should resolve to put off for Chicago, I feel it my duty to tell them, that she is already abundantly furnished with physicians. Of the exact number I cannot speak, but the *signs* of an adequte supply are conspicuous; and arrangements have been made for keeping it up, by home manufacture. Of this I gave you intimation in my last, expressing some disapprobation of the proposal to manufacture them here, and in some other places, at so small a cost. In the four days which have elapsed since the date of that letter, my opinions have undergone no change, and of course I still think, that as a cutler, who might advertise to make surgical instruments at half price, would not be patronized by our operators, so, surgeons made at half-price, should not, *a priori*, be regarded by the people, as strong and acute. Among the instruments made by the manufacturer of 'cheap goods,' there might now and then be one of excellent temper, and among the alumni of our cheap schools there may be found some of excellent professional temperament, but neitheir the shops nor the schools should be judged by these exceptions. Justice, however, to Dr. Brainard, the enlightened founder of the 'Rush Medical College' of this place, requires me to state,

that he himself, in the abstract, does not approve of cheapening medical instruction; but says he was driven into it by the example of the schools in this latitude, from Geneva, in New York, to Fox River, in Illinois, embracing the inermediate establishments of Willoughby, Cleveland, Laport, and Jacksonville. He and his colleagues, indeed, have it in view, in due time, to advance the price of their tickets. He does not anticipate a very rapid growth of his establishment, and would not, in the infancy of the country, have moved in its organization, but that several towns, which he justly regarded as too small and sequestered, had been made the sites of such institutions. I must confess that this view of the matter is plausible; and that if the patronage which would be distributed among the towns of Laporte, Jacksonville, and St. Charles, can be concentrated on Chicago, it will be for the benefit of the profession and society at large. Indeed, it must, I think, be admitted, that the towns just named, together with Willoughby, in the State of Ohio, are not places where flourishing medical colleges can be built. West of Pennsylvania and New York, leaving out of view the towns on or near the Ohio, the three points favoring and requiring such seminaries, are Saint Louis, Chicago, and Cleveland. The first is connected with the Lower and Upper Mississippi and with Western Illinois; the second has connection with Northern Indiana, middle and Northern Illinois, Western and Northern Michigan, Wisconsin and Iowa; the third may look to Michigan, North-eastern Indiana, Northern Ohio, North-western Pennsylvania, and Upper Canada; within which various States and Territories, there will, in another age, be a dense population, supplying students enough for three schools, but not for the *eight* which have already announced themselves. Each of the towns which I have mentioned, will have population enough to give business to a faculty of teachers, and subjects for the practical anatomist; each will have a hospital, where clinical medicine and morbid anatomy may be taught; and each will erect such literary and scientific associations, as will favor research and ambitious study, in both professors and pupils.

The first annual catalogue of the school of this place, just published, embraces 22 students, which is rather more than the first class in Transylvania University, or the Medical College of Ohio. and beyond what might have been expected on a spot, which 15 years ago was but a military post and the site of an Indian Agency.

I have made a visit to the edifice of the school—(to be finished in time for the ensuing course of lectures)—which will be commodious and respectable. The money with which the Trustees are building it, has been cheerfully contributed by the citizens of Chicago. The faculty have also commenced a *monthly*, under the title of the "Illinois Medical and Surgical Journal," of which Prof. Blaney is the Editor. Each number contains 16 large and well-printed pages the contents of which I have not had time to study, but observe a large proportion of original matter. Thus Chicago is fairly in the field, and her nearest rival will be St. Louis. I hope they will set an example to their seniors, of honorable bearing in the *certamen gloriæ*.

WISCONSIN TERRITORY, }
Milwaukee, Sept. 29. }

On the Western shore of Lake Michigan, there are four flourishing towns—Chicago, of which I have just spoken, in the State of Illinois, and South Port, Racine, and that in which I am now writing, in the Territory of Wisconsin. At South Port I did not stop, but in Racine I spent two days, and met with several intelligent physicians. I found here, as further South, that the last spring and summer, had been unusually wet, but the prevalence of autumnal fever not great. Of all the towns in East Wisconsin, that where I now am is the largest. Within its limits there is a great deal of lacustrine marsh, and yet it suffers but little from the fevers of autumn. This exemption is referable to two causes—first, the daily fluctuations of the Lake, by which the marsh is drenched; and second, the high latitude, 43°, giving a summer of too low a temperature to favor the development of malaria. At Green Bay, in lat. 44° 40′, I found, when I visited it in 1842, that intermittent and remittent fevers, are almost unknown, although many conditions requisite to their production are present. The climate overrules them. If I descend from these generalizations, to a few miscellaneous details, I hope you will not regard them as beneath the dignity of the vehicle in which I would send them abroad. First; the emigrant physician to these parts, should provide himself with Mr. Lapham's "*Geographical and Topographical description of Wisconsin*," a scientific and instructive little volume, just published. When the canal at Louisville was excavated, Mr. L. was a youth who car-

ried the leveler's rod; he is now the natural and statistical historian of this distant territory, and has given a very good account of its geography, geology, and botany, with notices of its climate and diseases, not unworthy the attention of the medical man, who may seek a residence here. Second; Milwaukie is destined to be a pretty town, from the *natural* hue, so to speak, of its bricks, which, when burnt, are of a light yellow, cream color; pleasanter to the eye, than the finest shades of Louisville or Cincinnati house painting, and apparently as hard and durable as bricks made of clay which burns red. Third; the physicians of Wisconsin and Northern Illinois, have a class of patients which we never see on the banks of the Ohio—immigrants from Norway; who in their anatomy and physiology, resemble ourselves more, I think, than the Germans do. They are very fair, with blue eyes and sandy hair. Fourth; but the glory of Wisconsin is not in its historians, its bricks, or its Norwegians, but its potatoes; on which a young doctor may pass his probation, with two cents a day, and come out of it in much better case, than the bear leaves his hybernaculum. These *meshannocks*, (I know not the classical orthography) would sell for a profit in the South, and the physician who might receive them in exchange for his boluses, would find it to his interest to ship them in that direction.

With this important annunciation, I will lay down my pen, and prepare for my journey across the Territory to the Mississippi river.

MADISON, October 3d.

Two days of stage travel has brought me to the capital of the Territory, where I propose to close my epistle. Its distance is nearly 100 miles from Milwaukie, its latitude a few minutes further north, being about 15′ north of 43°. It is beautifully situated, between the two upper of a chain of four romantic little lakes, which discharge their surplus waters into Rock river. The surrounding country, although thinly peopled, is fertile and fair in its aspects; the seat of Government it is expected will continue here after the Territory has passed through its chrysalis state; and the proportional number of physicians, I am told, is not great. This, therefore, would seem to be not an unfavorable spot for one or two good immigrant doctors, especially as the lakes abound in the finny

races; and the young practitioner might add fish to his potatoes,—a diet highly favorable to sound health, hard study, and patient waiting.

I am now on the confines of the 'Mineral Regions,' and shall make it the subject of my next.

Respectfully,

Your friend and colleague,

D. D.

PROFESSOR CALDWELL AND ANIMAL CHEMISTRY.

It is not our intention to protract the discussion on animal chemistry by replying to the long communication of our learned correspondent and friend, which is concluded in our present number. We have not been able to find that Professor Caldwell has brought forward in his Reply any material fact or argument not contained in his Critique, which we made the subject of an extended notice, and therefore do not perceive any necessity for an answer to his paper, even if we did not suppose that a disproportionate space in the Journal had already been given to the subject. It were easy to show that his attempt to point out the difference between chemical action and vital action is not successful; and it seems to us that it would not be difficult to prove, that in his comparison of the vital principle to the general of an army, and chemistry to the soldiers, who do everything under the direction of the commander-in-chief, he surrenders the main question between him and Liebig. If we understand Liebig, that is precisely his doctrine. The *vis vitæ*, according to him, presides over and modifies all the functions of living beings in which chemistry is concerned. We have so often quoted Liebig to this effect, that any further reference to his views on that point is deemed quite unnecessary. One more remark is all we shall stop to make concerning the reply of our venerable friend, and that is, that if our notice of his Critique was marked by undue severity, he has meted out the retribution according to his own measure. But we do not complain—we can bear with a great deal from our friends.

Y.

DR. M'DOWELL ONCE MORE.

When we made the exposure of Dr. M'Dowell's ignorance of diagnosis in our last number, we trusted that we should not again feel called upon to bring him before our readers, nor is it our purpose now to say much more about him; but as he has put forth a pamphlet, which may reach some of our friends, assailing us in language not known among gentlemen, and denying the truth of our report of one of the cases given in our last notice, we trust we shall be excused for devoting a little more space to him, and to his professional and literary pretensions. We challenged him, it will be remembered, to adduce the opinion of a single physician of science, that he had ever cured a case of consumption; and what does he do in reply? He parades certificates from men confessedly and necessarily ignorant of the symptoms of the disease, "*unmedical* men," as he styles them, "that he has cured cases which *he called* consumption;" and this he proclaims "indubitable testimony;" as if the nostrums of every quack in Christendom did not rest upon testimony precisely as indubitable!

The only reply we shall make to his denial of the accuracy of our history of his case, is the following certificate from Dr. Buck, one of the most accomplished young physicians in Louisville. He was present at the autopsy of the lady, to see verified Dr. M'D.'s diagnosis of her case. The following is his testimony:

LOUISVILLE, OCTOBER 28, 1844.

I state, at Dr. Yandell's request, that I understood from Mr. Willard a short time before the death of his wife, what I had repeatedly heard from Dr. Richardson as coming from Mr. W.—that Dr. M'Dowell considered the tuberculous matter as almost, if not entirely, removed from Mrs. W.'s lungs, the cavities having become cicatrized; that the disease from which she then suffered most was *pneumonia;* and that Dr. M'D. believed the mesenteric glands were tuberculous, and very much enlarged. I was present at the *post-mortem* examination, in company with Drs. Drake, Gross, and Richardson. On removing the sternum and ribs, a cavity occupying the entire upper lobe of the right lung, filled with tuberculous matter, was discovered; two or three smaller cavities were found in the left lobe, and the surrounding tissues were studded with tubercles. No cicatrix was discovered in either lobe, nor any sign of pneumonia. Dr. M'D., on seeing the state of the lungs remarked

that he had not heard *pectoriloquy* for six months! This I regarded as an expression of his surprise at finding the lungs in such a condition. The mesenteric glands were but little, if at all, enlarged—the promonotory of the spine having been mistaken for their enlargement. (Signed)

J. R. BUCK.

Now the reader will see by this statement, that, far from exaggerating in our report the blunders of Dr. M'Dowell's diagnosis, we suppressed some of the most disgraceful of them. We contented ourselves with showing that he could not detect the presence of tuberculous matter in the lungs, and forebore to exhibit him in the pitiable light of mistaking phthisis for pneumonia, and the promontory of the spine for the mesenteric glands. To such a depth of ignorance, we would hope, there are not many men in an honorable profession capable of descending, and if the exposure of it give him any pain, he has to thank his own folly for it, which induced him to question the veracity of our statement. We will only consume the time of the reader to remark further, that this pamphlet of Dr. M'D.'s shows him to be as incapable of writing the English language, or even of spelling common English words, as Dr. Buck has proved him to be of recognising a case of consumption when he has one before him. But, if he has not learned to write, or to spell, or to distinguish consumption from pneumonia, or the mesenteric glands from the back-bone, he may console himself with having studied figures to some purpose, and with understanding how to run up long bills against patients after other physicians have given them up to die. His art in this respect is consummate, as numerous cases, well known in this city, fully attest. Of such an author and practitioner, whose appeal has now been made from the profession to the people, we cannot be expected to have anything more to say.

Y.